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ON POSITIVE AND NEGATIVE ELECTRONS.

BY H. A. LORENTZ.

(*Read April 17, 1906.*)

The illustrious founder of our society will forever occupy a prominent place in the history of physical science, not only for his experimental researches in electricity and his invention of the lightning rod, but also for his theoretical views. Franklin tried to explain all electrical phenomena that were known in his time by means of a single electric fluid, which he supposed to be present in certain definite quantities in all ponderable bodies, when in their natural or unelectrified state, and in larger or smaller quantities in positively or negatively charged bodies. The rival doctrine was of two electric fluids, which in the days before Maxwell served as the foundation of the mathematical theory of electricity and which was adopted by those physicists who, like Riemann, Weber and Clausius, sought to account for electrostatic and electrodynamic phenomena by one fundamental law for the mutual action of electric particles.

Some twenty-five years ago the relative merits of the different laws of this kind that had been proposed, especially of those of Weber and Clausius, were examined by many physicists and in connection with this, there was much discussion about the motion of the two electricities in an electric current. Whereas, in the applications of Weber's law, they were generally supposed to travel

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in opposite directions with equal velocities, Clausius could free himself from this restriction, and could even suppose one of the electricities to have no motion at all.

These questions, after having for a time lost much of their interest by the universal spreading of Maxwell's ideas, have again sprung up, and have even become of fundamental importance in the modern theory of electrons. I should therefore like to call attention to them for a few moments, hoping the subject will be thought suitable on the present occasion, because it is somewhat like the old question whether one had to assume Franklin's single fluid or a positive and a negative electricity.

In order to show the connection I may observe that we can never wholly escape from the dualism, the notion of two things with opposite properties, that is forced upon our minds as soon as we come to study phenomena. Indeed, while recognizing but one electricity, the unitarian theory invested ordinary matter with the properties of the missing fluid. It was obliged to assume the existence of a mutual repulsion, not only between the particles of electricity, but also between those of matter and to add to these forces an attraction between a particle of matter and one of electricity. This is not very different from a two-fluids theory; it is even practically equivalent to it, if one of the two fluids is supposed to be permanently fixed to the ponderable matter. After all, we shall have to choose, not, strictly speaking, between one or two electricities, but between one or two *movable* electricities, in modern terms, between one or two kinds of *movable* electrons.

I shall confine myself to the case of metallic bodies and I shall first speak of a phenomenon which at first sight might seem sufficient to lead us to a decision.

Let us consider a very thin rectangular sheet of metal, traversed in the direction of its length, say from left to right, by an electric current, and placed in a magnetic field whose lines of force are perpendicular to the sheet. Let us first suppose the current to consist of a flow of positive electrons, towards the right-hand side, of course. Then, by a well-known rule, each of these electrons will be acted on by a force due to the magnetic field and perpendicular both to the lines of force and to the current. This force will tend to drive the

electrons in the direction of the breadth from one edge of the plate towards the other, so that, if two points of these edges, which would be at equal potentials in the absence of a magnetic field, are connected by a conducting wire, a current will be set up in the latter. A similar effect, but in the opposite direction, would be produced if the current were a flow of negative electrons. This is easily seen if we keep in mind that the motion of the negative particles must be supposed to be opposite to the nominal direction of the current and that the force exerted by a magnetic field on a moving electron remains the same if the sign of the charge and the direction of the motion are reversed at the same time. On account of their motion from right to left, the negative particles will therefore be driven towards the same edge of the sheet as the positive ones in the former case; the direction of the current produced in the connecting wire will therefore be reversed.

Having got thus far, we can also see what effect will be caused by the magnetic field if the current we send through the metallic sheet consists of a flow of both positive and negative particles in opposite directions, so that its intensity can be considered as the sum of those of two partial currents, i_1 and i_2. We shall then have a superposition of two opposite effects, either of which may predominate, according to the relative magnitudes of i_1 and i_2.

Now the phenomenon which the foregoing reasoning might lead us to expect, has really been observed. I need scarcely tell you that it was discovered by Professor Hall, then working in the laboratory of the Johns Hopkins University, at a time when there was hardly any question of a theory of electrons. The effect has been investigated for a large number of metals and has been found to have different directions in different substances. This is of especial importance in our discussion, for it seems to prove that we must indeed imagine two kinds of free electrons, the motion of the positive ones predominating in one body and that of the negative ones in the other.

I shall now point out some difficulties which present themselves in the further development of this conception of an electric current as a double stream of electrons. Take for instance the simple case of a current flowing across the junction of two pieces of different

metals M and M', say from the former towards the latter. Considering two sections, S and S', of the two metals quite near their surface of separation, I shall denote by n_1 the number of positive particles traveling across S per unit of time in the nominal direction of the current, by n_2 that of the negative ones going the other way, and by n_1', n_2' the corresponding numbers for the section S'. Then, if for the sake of simplicity we suppose all electrons to have equal charges, we shall have

$$n_1 + n_2 = n_1' + n_2',$$

but of course this does not imply that n_1 and n_2 are separately equal to n_1' and n_2'. If the Hall-effect is not the same in the two substances, the ratio between n_1 and n_2 will be different from that between n_1' and n_2'; it may very well be that n_1 is much larger than n_2 and n_1' much smaller than n_2'.

In order to fix our ideas, I shall suppose

$$n_1 > n_1', \quad n_2' > n_2.$$

This means that the number of positive electrons entering the space between S and S' through the first of these sections exceeds the number leaving it through the second, so that the number contained within the space will increase by $n_1 - n_1'$. As there will be an equal increase $n_2' - n_2$ of the number of negative particles the result is a continual accumulation at the junction itself of equal positive and negative charges, or, as we may say, of *neutral* electricity. Conversely, neutral electricity would continually be carried away from the place of contact, if the direction of the current were reversed.

It is further to be noticed that a change in the distribution of neutral electricity would even occur if we had no current at all; if there were two kinds of movable electrons, it would already arise from the causes which produce the phenomenon of contact electricity. As to these causes several hypotheses have been put forth, of which two may be briefly mentioned. In his celebrated paper on the conservation of energy, Helmholtz accounted for the difference of potential between two metals by means of certain attractive forces exerted at very small distances by the material atoms on the particles

of the electric fluids, or, as we are to say now-a-days, on the electrons; if, for instance, the positive particles are more attracted by the metal M' than by M, this will of course tend to produce a positive charge of the first metal. A wholly different explanation that has been proposed by Riecke and Drude is based on the assumption that the free electrons in a metal have their share in the molecular agitation by which we account for the phenomena of heat, going to and fro with velocities whose magnitude is a function of the temperature. The consequence of this heat-motion must be a certain equalization of the density (measured by the number of particles per unit volume) with which the electrons are distributed over adjacent parts of space. Hence, if at the same temperature the metal M contains a larger quantity of free positive electrons than M', the first metal will lose and the second will gain a certain number of them and the potential of M' will be made to exceed that of M.

We need not stop to consider in detail these theories; it will suffice to observe that, according to both, the causes which bring about the difference of potential are confined to a very thin layer near the surface of separation of the two metals. Now, whatever may take place in this layer, it is clear that the transfer of electrons from one body to the other will go on until the causes determining it are balanced by the difference of potential that is established. A state of equilibrium would soon be reached in this way if there were but one kind of free electrons. But if there are two, the case will be different. The causes by which the positive electrons are driven across the junction being quite distinct from those on which the flow of the negative particles depends, the value P of the difference of potential which is necessary for preventing a further transfer of the positive electricity will in general differ from the value Q that is required for stopping the current of negative electrons. Hence, as there is but one difference of potential, a true state of equilibrium can never exist, unless there be some other process that has not as yet been taken into account. The only state of things that could be attained by the motion of the particles we are now considering would be one in which the difference of potential has such a value, intermediate between P and Q, that the two kinds of electrons flow in equal numbers towards the same side. It would be a final state in-

asmuch as there would be no further change in the charges or the potentials, but could not be called a state of equilibrium because there would be a never-ceasing stream of neutral electricity.

The question now arises, in this as well as in our first instance, what will become of the accumulating neutral electricity? We cannot suppose this mixture or combination of positive and negative electrons to be absolutely nothing, so that it might be drawn from a body or heaped up in it for hours or days without any observable change in its properties. We are therefore compelled to imagine some new process by which the neutral electricity is carried back from the places where both positive and negative electrons are concentrated towards those from which they are traveling away. Moreover, it is easily understood that a hypothesis of this kind can only suit our purpose if the moving neutral electricity is *not* composed of free electrons. If it were, all sections of the metallic system would after all be traversed by the same number of positive particles and also by the same number of negative ones and this is precisely what we have begun by denying. Our conclusion must be that the neutral electricity is to be regarded as a real combination, in pairs for instance, of positive and negative electrons, a combination that is formed in one part of the system and is decomposed again in another part towards which it is carried by a kind of diffusion.

Though this is rather complicated, we could be ready to admit it, if in doing so, we could obtain a quite satisfactory theory. Unfortunately, this is by no means the case, for it can easily be shown that the state of things we have now imagined would be in contradiction with the second law of thermodynamics. Indeed, it may be taken for granted that combination of a positive and a negative electron will produce a certain amount of heat and that, conversely, heat will be absorbed if the electrons are separated from each other. If now, as we have been led to assume, neutral electricity were built up in one of two metals which are in contact with each other and decomposed in the other, heat would be continually developed in the first and consumed in the second body. By Carnot's principle this can never be the case in a system that is kept at a constant uniform temperature, as our two metals may be.

If I am right in making this last remark, and if it cannot be

invalidated by some new hypothesis, we need no longer continue our comparison of the two theories; we ought surely to give up all attempts to explain phenomena by the assumption of two kinds of movable electrons. I shall only adduce one argument more, which may be drawn from what is known of the so-called canal rays and the α rays of radioactive bodies. The positive electrons which constitute these rays have been found to have a mass of the same order of magnitude as that of the chemical atoms, a fact which lends a strong support to the view that in a metal the positive charges are rigidly fixed to the material atoms and that only the negative electrons can freely move over considerable distances.

As to the Hall-effect, which at first sight seemed to speak so strongly in favor of the two fluids theory, we shall have to examine whether it cannot be accounted for by the motion of negative electrons only. If we succeed in this, as perhaps we can by going somewhat deeper into the mechanism of the phenomenon than we have done in our somewhat superficial discussion of it, we shall after all come to a system of explanations much resembling Franklin's unitarian theory of electricity.

LEIDEN, April, 1906.

FORM ANALYSIS.

By A. A. MICHELSON.

(*Read April 18, 1906.*)

As a recreation in the midst of more serious work, I have been interested in the analysis of natural forms; and hoping that the results of this somewhat desultory occupation be not deemed too frivolous for so august an occasion, I will venture to present some illustrations[1] and generalizations which have occurred to me. I recognize that the subject is one whose adequate treatment would tax the best efforts of one who combined the insight of the scientist, with the æsthetic appreciation of the painter and the gift of language of the poet—and certainly I am lacking in all three—but especially in the power of adequate expression.

I had hoped that my contribution would at least have the merit of originality, but I find that many abler investigators have found a similar delight in this interesting field, and have expounded their ideas with a wealth of poetic imagery and of exquisite illustration such as I cannot hope to emulate.

Haeckel in particular has treated the subject of General Morphology in so exhaustive a fashion that it would seem futile to attempt to add anything of real interest or importance. This is most assuredly the case as regards luxurious wealth of illustration, and better cannot be done than to employ the "Art Forms in Nature" with grateful thanks to the man who has placed these within our reach.

Regarding his classification of forms there may be room for differences of opinion; and here I may venture to suggest some modifications for such a general scheme as shall include not only natural objects but also physical and mathematical forms such as interference patterns and graphs of analytical expressions.

[1] The paper was illustrated with lantern slides taken mainly from Haeckel's "Kunstformen der Natur."

Classification of Forms.

I. *Symmetrical.*

II. *Unsymmetrical.*

I. Symmetrical forms.

Congruence of parts by

A. Rotation through 180° (Odd Symmetry).

B. Reflection in plane (Even Symmetry).

C. Rotation + Reflection (Partial Symmetry).

1. Radial Symmetry. (Corresponding points equidistant from radiant.)

2. Axial Symmetry. (Corresponding points equidistant from a fiducial line.)

3. Plane Symmetry. (Corresponding points equidistant from plane.)

1. Radial Symmetry.

a. Central Symmetry. (Corresponding points on straight line through center.)

b. Ovoid Symmetry. (Radiant in axis but not central.)

c. Excentric Symmetry. (Radiant not in an axis.)

2. Axial Symmetry.

a. Centraxal Symmetry. (Corresponding points on same perpendicular through axis.)

b. Dorsiventral Symmetry. (Corresponding points not on same perpendicular.)

3. Plane Symmetry.

a. Triplanar Symmetry.

b. Biplanar Symmetry.

c. Bilateral Symmetry.

The foregoing general systems of symmetry might be carried out in greater detail. For instance, the central symmetrical forms might be subdivided into (1) Spherical, (2) spheroidal, (3) ellipsoidal, (4) monoclinal, and (5) triclinal. So also the triplanar symmetrical forms may be classified into (1) Isometric, (2) quadratic, (3) rhombic; and the centraxial forms according to the number of similar parts into which an equatorial section may be divided, ($n = 2, 3, 4, 5 \ldots \infty$).

This, however, may well be left open, so that specialists in the various branches of science may elaborate details in such a way as to best fit their respective needs.

It will be noted that the proposed system extends the idea of radial symmetry to include the vast array of natural forms which *radiate*, though not necessarily from a center of figure and which would otherwise be considered either unsymmetrical, or as having merely axial or plane symmetry.

Another point is the *explicit* recognition of "odd" symmetry. This kind of symmetry is admitted by the mathematician on the same terms as "even"—though not so generally by the biologist or the crystallographer. It is easy to show that the one is quite as logical as the other.

The definition of symmetry is necessarily arbitrary—as in fact is well shown by the varied significance attached to the term by biologists, crystallographers and mathematicians. The last are coming to recognize any form as symmetrical when any admitted essential character is unaltered by any specified operation. So broad a definition would scarcely emphasize what is generally understood by the term; but if the "operations" be restricted to (1) rotation through two right angles, and (2) reflection in a plane, a symmetrical form may be defined as one whose corresponding points are (*a*) equidistant from a point, a line, or a plane; and (*b*) in which congruence may be obtained by a single "operation."

If condition *a* is fulfilled, but not *b* (as in the case of triclinic crystals), or if more than one operation is required for congruence, the form may be said to have *partial symmetry*.[1]

II. Unsymmetrical Forms.

Forms exist in endless variety which do not fall under any of these divisions, whose beauty and interest may be fully as striking as the symmetrical forms.

These may be classified according to the following scheme, which is only provisional.

A. Rhythmic.

[1] If it were not for condition *a* the helix would have to be considered a symmetrical form; but *b* provides that it have partial symmetry.

B. Spiral.

C. Branching.

D. Scattered.

E. Irregular.

It would far exceed the limits of time and space to develop these in detail, but it may be mentioned that:

Rhythmic forms may appropriately be divided into *Line* rhythms, *Nets* or surface rhythms, and *Space* rhythms.

Further the " element " of the rhythm may be constant or (regularly) variable, and (in the case of linear rhythms) the line may be straight or curved.

Spirals may include flat, cylindrical or conoidal forms.

Branching forms may include all the well-known systems of arrangement of leaves and flowers, as well as the ordinary types of branches, to which may be added a number of arrangements peculiar to some of the lower organisms.

Scattered forms are such as are typified by the arrangement of the stars in space.

Irregular forms may be subdivided into Definite, Indefinite, Distorted.

In many cases the essential character of both symmetrical and unsymmetrical forms may be expressed in mathematical language; and even in the cases where such formulæ are only approximate expressions of the reality, such expressions may be the best and readiest means of ascertaining departures from the law.

Doubtless the biologist will look askance at what appears a grewsome array of mathematical symbols—at least until he finds how much less vicious they are than they look—but for the student of mathematics, there can scarcely be a more interesting and instructive practice for familiarizing himself with the properties of analytical functions, than to exercise his ingenuity in fitting the most appropriate expression to the forms he sees in Nature.

A further possibility in the expression of natural forms in mathematical language is the presentation of an ideal—so to speak—which the object is trying to embody; and the attempt to determine what this form would be under standard conditions would furnish an interesting and fruitful subject for research.

In contemplating the variety of exquisitely graceful forms which are exhibited in such abundance by the lower organisms, it is difficult to realize that their building up may be the result of the action of purely physical causes—and in truth it may be long before the theory of their formation is complete.

It is safe to say, however, that such general characters as symmetry of parts, rhythmic arrangements, systems of branching—which occur in strikingly similar fáshion in such widely dissimilar objects as vegetation, protozoa, crytals, even liquids—must have some very general and fundamental explanation.

In many of the simpler cases such explánations are almost self-evident. For instance, the very frequent occurrence of spherical or spheroidal forms may be accounted for by the action of capillary forces; and the regular segmentation figures shown in many protozoan cells may be explained by the geometrical necessities of the case. An experimental study of such geometrical relations for figures in one plane was published by Alfred Mayer, and his results furnish many suggestive analogies which bear on the structure of the molecule as well as that of the micro-organism. An extension of the results to three dimensions would doubtless encounter serious experimental difficulties; but these may be overcome, or at least reduced sufficiently for practical purposes. The problem is really however a geometrical one—though the figures resulting from a purely mathematical investigation would be complicated by considerations of stability.

As an instance in which the cause of regularity is reasonably certain, let us consider the forms produced by a drop of colored liquid falling into a body of liquid of nearly the same density. As the drop descends it flattens out, the edge advancing more rapidly than the flattened surface. The equilibrium, already unstable, is still further disturbed by the momentum acquired by the peripheral parts, which causes them to assume a bell-shape. This form is still unstable, and if any accidental cause starts a thickening into drops, this will be assisted by the thinning of the adjacent parts which then lag behind. The same forces act at other points on the bell, to form "drops," the distance from the other drops being just enough to save being thinned. This distance (which depends on

the properties of the liquid) determines the " period " of the rhythm. The drops or loops once formed produce vortex motions in the adjacent liquid which tend to separate them, and the action of these quasi repulsions is the stronger the closer the drops are to each other. These will consequently separate until the " repulsions " are equal—that is, till they are equidistant.

The same general explanation holds if the original drop is transformed into a ring.

The result is a beautifully regular (but inverted) dichotomous (tri- or tetrachotomous) branching, closely resembling vegetation.

The study of symmetry is doubtless of relatively subordinate importance from the point of view of the specialist—though Haeckel rightly insists on its significance in the evolution of the lower organisms.

In the study of crystallography considerations of symmetry are of high value; and a clear conception of relations of symmetry often wonderfully simplify the most abstruse problems in mathematical physics.

In the construction of buildings and bridges and engineering works in general as well as in machinery and scientific apparatus, and even of common tools and utensils,—the very necessities of the case have enforced a more or less complete symmetry of parts and of the whole.

Indeed, it is a common experience that the design of a piece of machinery may be so altered as to make it symmetrical often with a surprising increase in efficiency as well as beauty.

In designing for the sake of decoration, symmetrical forms are everywhere manifest, and the perception of their mutual relations is indispensable to the student of art. Occasionally, however, there is in decoration a deliberate departure from symmetry, and such a variation may greatly enhance the beauty and effectiveness of the design. We tire of too great uniformity even of agreeable kinds, and the element of variety is as important in art as an occasional discord is in music—its purpose being to heighten the effect of the succeeding harmony.

One of the great disadvantages of the modern tendency to extreme specialization in research is the loss of companionship of

the sister sciences, with the attendant loss of perspective which a more general survey of the whole field of science should furnish. Should we not, then, utilize every opportunity which promises to further their union?

The geologist, the chemist, the physicist, the mathematician, may and occasionally do meet here on the common ground of crystallography. By a comparatively slight extension, the "ground forms" of organisms—as Haeckel terms them—may also be included with a corresponding extension of our society of sciences to include zoölogy and botany.

Nay, Art will demand a chair at the banquet, and Music and Poetry will also grace the feast.

BENJAMIN FRANKLIN AS METEOROLOGIST.

By PROFESSOR CLEVELAND ABBE.

(*Read April 20, 1906.*)

I have been requested by the chief of the Weather Bureau to represent him at this bi-centennial celebration and to express the profound respect that we have for Benjamin Franklin as the first meteorologist of America. It is true that we have records made by observers of the weather before he began his scientific activity, but the progress of meteorology has been such that we have now learned to put the philosophical investigator, that is to say, the man of research, far above the mere observer and recorder. Considered as a mere chronicle of passing events the study of the weather dates from the earliest ages; but considered as a rational investigation into its ultimate physical causes, or as the logical application of well established principles to the elucidation of unexplained phenomena, or as a system of research checked at every step by observations and experiments, the modern physical meteorology or theoretical meteorology, or dynamic meteorology deals exclusively with force or energy, and dates from the days of Galileo, Sir Isaac Newton, Huyghens, Descartes, Boyle and Gay-Lussac, with whom Benjamin Franklin was a worthy co-laborer.

At the present moment we are apt to apply the term "scientist" to one who devotes himself almost exclusively to some special department of research; but in Franklin's day those who contributed to research were generally occupied most of the time with other work, and this was notably true of him.

It is often remarked that events happening in one's childhood, or even a few years before one's birth, may produce such a profound impression on the whole community as to affect the general trend of one's thought and life; therefore, since Franklin did, from early youth, turn his attention so strongly toward natural phenomena, it may be worth while mentioning the fact that he was born in

January, 1706, and that shortly before this, on the 26th November, 1703, there was a most awful and destructive storm on the coast of England, in which a great number of merchants' vessels and crews were lost, as also thirteen sail of British men-of-war with fifteen hundred and nineteen men. This is the storm celebrated by Defoe's description, and to this day it must arouse the attention of every reader. Franklin was a child when in December, 1708, there occurred the coldest month experienced in New England up to that time, but I believe that we have no thermometric records for that time, as accurate thermometers, those made by Fahrenheit, were first brought to this country in 1720 by Dr. John Lining, of Charleston, S. C.

The various published editions of Franklin's writings have brought to public attention such a great variety of remarkable works that we are apt to think of him as a statesman, a printer, a philanthropist, an electrician and a patriot; but a careful study of his life and an examination of the great mass of unpublished manuscripts have shown that if he had done nothing else but his work in meteorology that alone would have entitled him to the highest rank. On this subject he thought and wrote for sixty years, from his diary of 1726 to his long range forecasts of 1786.

In the course of a study of old Philadelphia records, my attention has often been called to the profound influence exerted throughout this country and Europe by this great man. This present date is in fact the anniversary of his death and burial, yet we celebrate his birth and life, for he cannot be said to be dead and gone while the spirit that animated him still lives; while his influence is still felt; while his example is ever before us; while his words are repeated daily; while his maxims are the mottos of our own lives; while his monuments are everywhere and still increasing around us. He marked out the rules of life that will be followed for all time by reasonable men.

Right here let me stop to controvert an error that I have heard repeated today notwithstanding the most beautiful and truthful oration of yesterday by the Hon. Hampton L. Carson. I am told in effect that Franklin was not a religious man. There could not be a more monstrous error than this; it could only have emanated from

those who never knew him personally, but in distant places and distant years had heard repeated some of his pointed satires against superstition, dogmatic theology, priestly craft, hypocrisy and the union of church and state. To all such perversions of pure religion he was a lifelong enemy. Bred for a preacher and theologian; educated for the pulpit of the Church of the Puritans; thrown by his removal to Philadelphia into the companionship of honest Quakers; transferred to England to battle for Freedom from corrupt aristocratic government with its Established Church and persecuting clergy,—he, of all men, had occasion from childhood to investigate the foundations of our faith; he had learned to distinguish between that natural religion that is revealed in the heart of every good man, and the dogmas, forms and ceremonies by which the established union of church and state was controlling a large portion of Europe, England and America. His common sense rebelled against this alliance of error with power; he would have none of it. He believed in the Fatherhood of God; in a religion that should be a blessing to men; in an honest statement of what we know, as distinguished from what we imagine or believe. He searched the foundations of his own faith as carefully as he examined into the phenomena of nature; he applied the same rational logic to them both, and sought by independent thought and clear views to emancipate his friends and countrymen from abject subserviency to dogmatic teachers, who were, as he believed, blinded by their own ignorance, fettered by conservatisms and beguiled by love of power and ease.

Being familiar with every line of the Holy Scriptures, far more so than many of his adversaries, and knowing how they misused the words of the Bible, he turned it against them, and by such satires as his proposed new version of the Bible, in 1760, held up to the severest public contempt the false courtiers that surrounded the king, thus goading them on to further acts of injustice and selfishness. In every case in which he seems to be violating all habits of reverential religious thought, you will, if you understand the circumstances of the occasion, find that he was, as it were, confounding his enemies out of their own mouths. Read his "Shavers and Trimmers" in the *Pennsylvania Gazette* for 1743, or the "Parable against Parsons," or the "Parable on Brotherly Love," and learn that there are

shavers and trimmers at court, at the bar, in church, and in state.

Now turn to his "Articles of Belief and Acts of Religion" in the reprint by Smyth, vol. 2, pp. 91–100, and then recall that this elaborate declaration of the very highest form of natural religion, fit to be reproduced in every prayer book and Bible the world over, is the earliest autograph we have from Franklin, being written by him at Philadelphia, November 20, 1728, and was his daily pocket companion to the end of his life! He had been back from London two years and had gone through many trials, "but God was with Joseph." This composition marks an epoch in his early life corresponding precisely to what is frequently spoken of as "conversion"; but it was a conversion from men's devices to the higher and larger spiritual "republic of God." This "Articles" is a document that can be read only with the deepest reverence and affords us the true measure of the religious inner life of the noblest of men,—Benjamin Franklin. It fully explains how it happened that from 1753 to the end of his life Franklin was the most intimate companion and friend of "the good Bishop of Asaph."

If we systematically search for evidences of Franklin's interest in meteorology we shall find that in 1726, on his return voyage from London to Philadelphia, he kept a regular diary including items of wind and weather, noting, for instance, on August 30, 1726, "the full moon with a rainbow in the cloud to the west of us, the first lunar rainbow I have ever seen." Again, on September 14, "about 2 P. M. a solar eclipse covering ten-twelfth of the sun's diameter"; and on September 26th, "a sudden squall and rain, but the wind backed to northeast." Again, on September 28th, "run into gulf weed with barnacles and crabs" such as he seems never to have seen on the seashore, he even kept living specimens to see if more such crabs would hatch out, noting that they resembled the crabs he once saw in salt water at Boston and Portsmouth. On September 30th he sits up to observe the eclipse of the moon, which he says "occurred at 5 A. M. September 30th by the London Calendar, but at 11 P. M. September 29th to 2 A. M. September 30th by local ships' time," hence he calculates "that we are now four and a half hours, or 67 degrees, west of Greenwich." October 2d he noted that the water was changing color as though they were near soundings, it changed

back again to blue on the 7th; and on the 11th they anchored at Red Bank, six miles from Philadelphia.

Having become proprietor of the *Pennsylvania Gazette* in October, 1729, he published therein an article on earthquakes (compiled, of course, from many sources, to which he gives full credit), as he says, "because of the interest excited in the subject by our recent earthquakes," alluding to slight local earthquakes of 1725 and 1726, that were felt throughout the allied colonies and roused many queries and fears. His editorial work stimulated his habits of reading and writing. He made notes of everything that could be useful to his readers. . . .

In 1726, August 21st, occurred the destruction of Peruna, Italy. In 1740 occurred the great earthquake at Lima. In 1750, that at Concepcion in Chili. On November 1, 1755, a part of Lisbon was swallowed up by an earthquake, sixty thousand persons perished in a few minutes and the ocean flowed over the site of the disaster. In 1755, October 30th, Damascus was destroyed and twelve thousand lives were lost. In 1760, November 25th, Tripoli was destroyed. These were the great earthquakes to which attention was forcibly directed during his early manhood, and such events must be associated with his own intellectual activity.

From 1732 to 1757 there were 25 annual issues of his "Richard Saunders Almanack." Now while it is true that in these he published conjectures as to the weather during the respective years, yet we are not to think of Franklin as a planetary meteorologist; for the fact is that in every one of these issues he disclaims all knowledge of the weather or astrology and pokes fun at his own predictions as utterly absurd and useless. He gives them, as he says, for what they are worth as food for reflection, as matters that are an essential part of a farmer's almanac,—not based upon any knowledge of nature but simply fanciful conjectures. They served, of course, to make his almanac sell, but they were not put forth in any serious vein of thought, but were really to him a matter of humor and fun. For instance, in his preface to his almanac for 1738 he makes "Mystriss Saunders" apologize for the imperfections of the almanac owing to the sudden departure of the husband, "poor Dick"; she adds, "upon looking over the months I see he has put in

abundance of foul weather this year and therefore, I have scattered here and there where I could find room, some fair, pleasant, sunshiny, for the good women to dry their clothes in. If it does not come to pass according to my desire I have shown my good will, however, and I hope they will take it in good part." I believe that all the prefaces to Poor Richard's Almanac have been reproduced in Smyth's edition of the writings of Benjamin Franklin, and their humor is so telling that one may well wonder why the most credulous person in those days should have imagined that there could have been anything serious or reliable in such almanac forecasts. Nothing can be more ironical and comical than the following illustration from the preface for 1739: "Ignorant men wonder how the astrologists foretell the weather so exactly unless we deal with the old black devil, alas! it is as easy as is this. The star gazer peeps at the heavens through a long glass; he sees perhaps Taurus or the great Bull in a mighty chafe stamping on the floor of his house, swaying his tail about, stretching out his neck and opening wide his mouth. It is natural from these appearances to judge that this furious bull is puffing, blowing and roaring. Distance being considered, and time allowed for all this to come down [to the earth]—there you have wind and thunder."

But Franklin's life was devoted to the work of promoting useful knowledge and abolishing ignorance, superstition and credulity. His guiding principles were close observation and logical experimentation, and as these succeeded perfectly in his hands in all manner of business and in his dealings with mankind, he did not see why they should not hold good in dealing with nature. His proposal of May 14, 1743, to form a society of ingenious men from all parts of the colonies, to maintain a constant correspondence, and his offering of himself as secretary to that great undertaking, involved such an expenditure of time, thought and money, that we may well consider him as the father of coöperative systematic research in this country, even though a similar idea had animated the Royal Society of London for many years before. He had freed himself from the trammels of theological connections, he was not afraid to see the truth and state the truth; he recognized the limitations of man, that we can not possibly know for a certainty anything except by personal

observation and that what we accept on the testimony of reliable witnesses must be capable of being tested by actual experiment or observation and by logical reasoning at any moment. He proposed that his new society should consider a long list of subjects mentioned by him specifically. This list did not include the weather or the atmosphere except by implication in the last paragraph, which reads thus: "and all phylosophical experiments that let light into the nature of things, tend to increase the power of Man over Matter and multiply the conveniences or pleasures of life." (Smyth, vol. 2, p. 230.)

As we delve into the great store of manuscripts relating to his life that have been preserved for us by his own habitual carefulness, we find new points of contact between him and modern ideas. His long record as a patriot, as an ambassador, as a master of pure English, as an electrician, as a man of the world, as a devoted brother, husband and father, has served to obscure some other equally prominent features of his character and his activity. His brilliant discovery of the identity between lightning and the ordinary electricity of the electric machine was not a so-called stroke of genius, but the inevitable result of his most persistent habit of thought and study, namely the determination that every idea that should occur to him of a philosophical nature should as soon as possible be tested by experiment. Accordingly we find the following memorandum in his daily note book, for November 7, 1749; after enumerating several analogies between lightning and the ordinary electric flash he adds: "The electric fluid is attracted by points; we do not know whether this property is in lightning; but since they agree in all the particulars wherein we can already compare them, is it not probable that they agree likewise in this? Let the experiment be made."

This was in November, 1749, at Philadelphia, at the season when lightning was rare, but he prepared to make the experiment as soon as practicable, doing it quietly in order to avoid attracting public attention, for a crowd was very apt to gather about him wherever he appeared. He at first planned to use a pointed rod on a high building,—afterwards he thought of attaching this rod to a kite. His silk kite had a pointed steel bar attached to the kite line, the latter was insulated by a silken cord and a key was tied at the junction, so

that all the details imitated his laboratory experiments. It is evident that he did not expect any very dangerous shock, or shall we rather conclude that knowing the poor conducting power of the wet kite string, he know that it would be unable to bring down any really dangerous amount of electricity, and that he therefore would not be risking his life when he touched his finger to the key and received those first delightful sparks of natural electricity from the clouds.

From boyhood Franklin had been accustomed to drill himself in all the arts of a good debater. Whatever subject was up for discussion he kept it in mind during his daily work and jotted down on opposite sides of a sheet of paper the arguments *pro* and *con;* the considerations that should have weight or none; the reasons for thinking this way or that. After crossing off all unimportant matters he would reduce the whole argument to a few terse sentences arranged in logical order. Precisely the same course was followed when in later years he turned his attention to natural philosophy. He tells us that having become prosperous in business by strict logical attention to every consideration that made for success, he, in 1747, believed himself to be in a position to retire from active business. He therefore engaged Mr. Hall as his partner, to take most of the labor of the concern off his mind. He built himself a new house, bought the electrical apparatus of Dr. Spence, of Boston, and added other pieces and proposed to devote himself to a life of philosophical inquiry, of literature and elegant ease. In this he was disappointed, as he writes many years afterwards. "My neighbors, finding that I was a man of leisure, insisted upon my carrying on many public duties, and finally sent me to the Assembly; so that my philosophical inquiries were greatly interrupted." His scientific reputation has, in fact, rested largely upon the work that he did in Philadelphia and London between 1747 and 1757, much of which was published many years later. At the close of that period his correspondence, observations and notes on experimental work seem to have been boxed up previous to his starting for London on his first mission for the people as against the Proprietors. Eventually his collection was removed for safety to the country outside of Philadelphia; a part of it was lost during the revolutionary troubles, and

another portion since then; but much has survived, and by searching over the fragments that remain we find that Franklin was always inquiring into the ultimate causes, or as we now say, the physical causes, of natural phenomena. We sometimes speak of him as an electrician, but before he studied electricity and in fact during his whole life he was seeking for explanations of atmospheric phenomena, so that the records will bear me out in asserting that he was among the first and best of the meteorologists, as distinguished from the climatologists, of his day. He tested the forces of nature and searched the motives of men. As his barometer and three thermometers were always at hand in his library, and as he refers frequently to the atmospheric pressure and temperature I shall not be surprised to find some regular record of these elements among the mass of his manuscripts that have yet to be examined, and I am hopefully hunting for his earliest records of the climate of Philadelphia. All are familiar with his inventions of improvements in the barometer, thermometer, and hygrometer; all know his important position as the discoverer of the progressive movement of the northeast wind and rain advancing from Georgia to New England. We must not forget that as postmaster at Philadelphia, 1737–53, and Postmaster General for all the colonies (1753–1775), he was traveling north or south incessantly, and handling reports from his local postmasters that showed him the conditions of the roads and the weather throughout the whole extent of his postal routes. No man was more familiar than he with all the current features of the country as to weather and crops, with the people and their mutual relations to each other, with the business and resources of the country. His argument as to the movement of the storm of 1743 was not a suspicion or guess, but a perfectly sound conclusion based on special inquiries and reports; it was precisely what we now call a careful research. I have collected many forgotten items that will go far to establish his reputation for solid work as a meteorologist, but perhaps the most interesting paragraph, and the only one I need here quote, relates to what are now called long-range forecasts of the weather and the seasons. Although he had published his own "Poor Richard's Almanac" for twenty years with its conjectures as to the weather, based on planetary configurations, yet in every al-

manac, without exception, he had ridiculed the whole subject, while at the same time knowing that popular superstition and ignorance would cause his almanac to sell. His own ideas as to such forecasts were far above those of his readers, and honest inquiry in this direction was always near his own heart. In the last years of his life he thought he saw one rational connection between cold winters in Europe and the foggy character of the preceding summers. This led him to write a very conservative memoir dated at Passy, near Paris, May, 1784. It was published in the Memoirs of the Manchester Society and I reproduce it from Writings of Benjamin Franklin (Sparks), Vol. VI, pp. 455–456, (Bigelow) Vol. III, p. 488, as follows:

METEOROLOGICAL IMAGINATIONS AND CONJECTURES.

PASSY, May, 1784.

There seems to be a region high in the air over all countries, where it is always winter, where frost exists continually, since in the midst of summer, on the surface of the earth, ice falls often from above, in the form of hail.

Hailstones, of the great weight we sometimes find them, did not probably acquire their magnitude before they began to descend. The air, being eight hundred times rarer than water, is unable to support it but in the shape of vapor, a state in which its particles are separated. As soon as they are condensed by the cold of the upper regions, so as to form a drop, that drop begins to fall. If it freezes into a grain of ice, that ice descends. In descending, both the drop of water and the grain of ice are augmented by particles of the vapor they pass through in falling, and which they condense by coldness, and attach to themselves.

It is possible, that, in summer, much of what is rain when it arrives at the surface of the earth, might have been snow when it began its descent; but, being thawed in passing through the warm air near the surface, it is changed from snow into rain.

How immensely cold must be the original particle of hail, which forms the centre of the future hailstone, since it is capable of communicating sufficient cold, if I may so speak, to freeze all the mass of vapor condensed round it, and form a lump of perhaps six or eight ounces in weight!

When, in summer time, the sun is high, and continues long every day above the horizon, his rays strike the earth more directly, and with longer continuance, than in the winter; hence the surface is more heated, and to a greater depth, by the effect of those rays.

When rain falls on the heated earth, and soaks down into it, it carries down with it a great part of the heat, which by that means descends still deeper.

The mass of earth, to the depth perhaps of thirty feet, being thus heated to a certain degree, continues to retain its heat for some time. Thus the first snows, that fall in the beginning of winter, seldom lie long on the surface,

but are soon melted, and soon absorbed. After which, the winds, that blow over the country on which the snows had fallen, are not rendered so cold as they would have been, by those snows, if they had remained; and thus the approach of the severity of winter is retarded; and the extreme degree of its cold is not always at the time we might expect it, viz., when the sun is at its greatest distance, and the day shortest, but some time after that period, according to the English proverb, which says, "As the day lengthens, the cold strengthens"; the causes of refrigeration continuing to operate, while the sun returns too slowly, and his force continues too weak, to counteract them.

During several of the summer months of the year 1783, when the effects of the sun's rays to heat the earth in these northern regions should have been the greatest, there existed a constant fog over all Europe, and great part of North America. This fog was of a permanent nature; it was dry, and the rays of the sun seemed to have little effect towards dissipating it, as they easily do a moist fog, arising from water. They were indeed rendered so faint in passing through it, that, when collected in the focus of a burning-glass, they would scarce kindle brown paper. Of course, their summer effect in heating the earth was exceedingly diminished.

Hence the surface was early frozen.

Hence the first snows remained on it unmelted, and received continual additions.

Hence perhaps the winter of 1783–4, was more severe than any that happened for many years.

The cause of this universal fog is not yet ascertained. Whether it was adventitious to this earth, and merely a smoke proceeding from the consumption by fire of some of those great burning balls or globes which we happen to meet with in our course round the sun, and which are sometimes seen to kindle and be destroyed in passing our atmosphere, and whose smoke might be attracted and retained by our earth; or whether it was the vast quantity of smoke, long continuing to issue during the summer from Hecla, in Iceland, and that other volcano which arose out of the sea near that island, which smoke might be spread by various winds over the northern part of the world, is yet uncertain.

It seems however worth the inquiry, whether other hard winters, recorded in history, were preceded by similar permanent and widely extended summer fogs. Because, if found to be so, men might from such fogs conjecture the probability of a succeeding hard winter, and of the damage to be expected by the breaking up of frozen rivers in the spring; and take such measures as are possible and practicable, to secure themselves and effects from the mischiefs that attend the last."[1]

Franklin's argument may be condensed as follows: The soil is warmed during summer, and its heat comes back to warm the air during winter. Its effect on the air is felt until snow accumulates

[1] See Sparks, "Life of Benjamin Franklin," Vol. 6, 455–457.

on the ground, so that the severest cold is delayed until long after December 21st. The fog of the summer of 1783 cut off the sun's heat, and left the earth cold; hence early snows were not melted; hence the cold began earlier and became more intense. If in future years a similar summer fog occur, it may be safe to predict that a similar cold winter will follow.

Two years after this we find the following two letters,[2] from his sister, Mrs. Jane Mecom. The first is dated Boston, December 17, 1786: "Your predictions concerning a hard winter are beginning to be verified in a formidable manner. The snow has been so deep that we might have been buried alive were it not for the care of some good neighbors"; and again, her letter dated March 9, 1787: "Your prediction has held invariable thus far, and as it began in October I don't see why it may not hold until May, for any appearance yet to the contrary." Must we not infer from this that Franklin had actually sent her a prediction for a cold winter beginning with October, 1786? The letter by him containing this prediction has not yet been found, but we may be sure that when found it will have in it nothing of planetary meteorology and nothing but strictly logical conclusions based on some well-established facts. I am hunting for this lost letter, but meanwhile I add to the laurel that crowns him another leaf, as the pioneer of the rational long-range forecasters, and of the physical meteorologists who will, undoubtedly, in the future develop this difficult subject.

[2] These were first published at page 151 of the "Letters to Benjamin Franklin from his family and friends, 1751–1790," "author, W. D."; published by C. B. Richardson, 348 Broadway, New York, 1859. These letters had passed from William Temple Franklin to Dr. Franklin Becke.

WAS LEWIS EVANS OR BENJAMIN FRANKLIN THE FIRST TO RECOGNIZE THAT OUR NORTHEAST STORMS COME FROM THE SOUTHWEST?

By WILLIAM MORRIS DAVIS.

(*Read April 20, 1906.*)

The account of the "Middle British Colonies in America," prepared by Lewis Evans and published in Philadelphia in 1747, contains a remarkably clear and appreciative description of the main features of New Jersey and Pennsylvania. It is illustrated by a map which, like the text, bears witness to an extraordinary acuteness of observation and as well to an unusual power of generalization on the part of the author, who must be ranked as an early leader among American geographers. The map contains a number of explanatory legends, inserted where topographical details were wanting; and here we find, among various items, a significant statement regarding the movement of storms: "All our great storms begin to leeward; thus a NE storm shall be a day sooner in Virginia than in Boston." This brief statement has been taken to be the first recognition, as it surely seems to be the first published announcement of the progressive movement of storms, on which so much of the modern art of weather prediction depends. The statement is however not easily accessible to citation, for apart from the great rarity of complete copies of the first edition of Evans' essay—the map being lost from some of the few copies known to me—the map in the second edition was amended by replacing some of the legends with newly gathered topographical data; and among the matter thus removed was the statement above quoted concerning the movement of storms.

Evans' publishers were Franklin and Hall, and there is good reason to believe, as has already been pointed out by students of this question, that it was Franklin and not Evans who provided the statement concerning storms, along with some account of lightning and electricity, subjects with which Evans was not particularly

concerned, but with which Franklin was much occupied. A reference to the letters in Sparks' "Life of Franklin" leaves no doubt on this point. In the very year of the publication of the first edition of Evans' essay, Franklin wrote to a friend as follows:

"We frequently have along the North American coast storms from the northeast, which blow violently sometimes three or four days. Of these, I have had a very singular opinion for some years, *viz:* that, though the course of the wind is from northeast to southwest, yet the course of the storm is from southwest to northeast; the air is in violent motion in Virginia before it moves in Connecticut, and in Connecticut before it moves at Cape Sable." It is a condensed duplicate of this statement that appears on Evans' map.

A question later arose as to the date when this "very singular opinion" had been formed. It was thus brought out that the observations which led to the opinion were made in connection with an eclipse of the moon, which Franklin failed to see in Philadelphia because the sky was covered with the clouds of a northeast storm, and yet which his brother, Thomas, successfully observed in Boston, where the northeast wind and its clouds did not occur until some hours after the eclipse. From this little hint, which many an investigator might have allowed to pass by without further consideration, Franklin gained the idea that the storm might possibly work its way against the wind; and this idea he confirmed by writing to a correspondent in Virginia, from whom he obtained the report that the storm there had begun sooner than it had in Philadelphia. The date of the lunar eclipse was afterwards determined to be in the year 1743. It would thus appear that Franklin contributed a statement of his discovery to Evans' map, making no claim whatever for recognition or priority; and indeed, suffering the statement to be obliterated without remonstrance, so far as now appears, when the second edition of the map was published. Generous as he thus showed himself, to the point of indifference, it is still fitting that we at this time should take pains to give credit where credit is due.

CAMBRIDGE, MASS., April 18, 1906.

PYRITE FROM CORNWALL, LEBANON COUNTY, PENNSYLVANIA.

By CHARLES TRAVIS.

(*Received June 22, 1906.*)

Several varieties of pyrite have been described from the Cornwall magnetite mines. Dr. Carl Hintze[1] mentions three,—(1) a combination of (111) and (100) without a reference; (2) a combination of (100), (111), (210), (321), (432), (221) containing 2 per cent. cobalt;[2] and (3) another variety for which no forms are given, but whose analysis shows 2.39 per cent. copper.[3]

The present paper is a crystallographic study of two varieties of Cornwall pyrite, which will be called Type I and Type II respectively. Type II is identical with the second variety of Hintze, the cobaltiferous; a rough analysis showed 1 per cent. cobalt, and all of the above given forms except (432) were found. Type I, on the other hand, shows no copper nor cobalt, and cannot be identified with any of the above. Apparently it has not been described before.

Occurrence.—Type II occurs scattered through the ore body itself. Crystals of this type grow in cavities in the magnetite, or in what apparently once were cavities. Byssolite and chlorite are commonly found with them. Where the ore is lean, the pyrite crystals are more numerous and are arranged roughly in layers.

The magnetite rests against limestone, which is altered at the contact. Just beyond the contact, in the limestone, Type I occurs, certain thin layers in the rock near the contact and parallel to it being rich with pyrite of this type. The crystals are so crowded in these layers that they largely interfere with each other's growth and perfect crystals are rare.

[1] "Handbuch der Mineralogie," Vol. I (1900), p. 964.
[2] Blake in Dana's "Mineralogy" (1868), p. 63.
[3] Booth in Dana's "Mineralogy" (1855), p. 55.

Type I.

In habit Type I is a combination of (111) and (210), (111) being more developed. The pyritohedron (210) appears in all degrees of development from tiny triangular faces at the corners of the octahedron to equilibrium with it. The trisoctahedron (221) is common and seems to vary with (210), those crystals that have a small pyritohedron face having little or no trisoctahedron. This is not, however, true in all cases. The cube is comparatively rare.

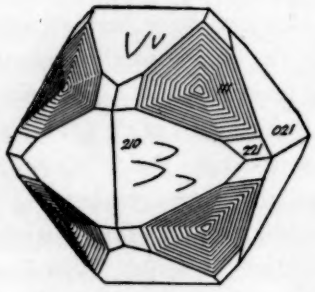


Fig. 1. Type I.

The faces in general are fresh and brilliant. Crystals from the weathered portions show a brown tarnish, which is removed by hot hydrochloric acid, leaving the crystal as bright as fresh material. The octahedron is striated parallel to its intersection with (210) and (221), producing six-sided figures. The pyritohedron has growth figures on its surface, in the form of curved isoceles triangles, whose sides are apparently the intersection of (210) with all possible trisoctahedrons from (221) to (111).

The cube, when present, is the most perfect face on the crystal, and is absolutely without striation. This may be partly due to the fact that it is never very large, and is usually a narrow face beveling the edge of the pyritohedrons. Fig. 1 illustrates the general habit of Type I, and makes an attempt to show the striation and growth figures. The crystals of Type I are, in general, small, few exceeding 0.5 cm. in diameter.

On a series of fifteen crystals of Type I the following forms were observed. Those marked (*) are new for pyrite, and one (14.11.8) has been reported from but one other locality, namely Kotterbach.[1]

[1] K. Zimanyi in Groth's *Zeitschrift*, Vol. 39 (1903), p. 125.

	Trisoctahedrons.	Diploids.
(100)	(331)	(531)
(111)	*(11.11.4)	(321)
(210)	*(552)	*(753)
	*(773)	(432)
	(221)	(14.11.8)
	*(774)	(421)
	(553)	(13.7.3)
	(332)	*(952)
	*(443)	*(14.9.3)
	*(554)	*(542)
	*(665)	

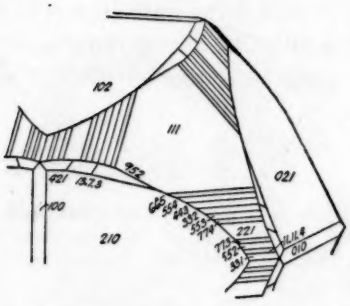


FIG. 2. Type I. Showing trisoctohedrons.

The observations determining these forms are as follows:

(331) was observed on crystal 2 with two faces in the same zone. The better one is about 0.1 mm. wide, and gives the measurement

331 ∧ 221.

Measured	6° 07′
Calculated	6° 12½′

The second face gives a much poorer image,—

Measured	5½° about; signal very poor.

(11.11.4), a new form for pyrite, was observed on crystal 7 with two faces, each about 0.2 mm. wide.

11.11.4 ∧ 111.

Measured	20° 48′
Measured	20° 51′
Calculated	20° 54′

(552), a new form for pyrite, was observed on three crystals, numbers 6, 7 and 10. Numbers 7 and 10 afford the best measurements.

552 ∧ 111.

Measured	19° 25′	(No. 7.)
Measured	19° 10′	(Two faces on No. 10)
Measured	19° 34′	(Two faces on No. 10)
Calculated	19° 28′	

The measurements on crystal 6 are near enough to identify the form after its presence has been established by the above,—

Measured	19° 03′
Measured	19° 02′

(Two faces on No. 6, measured by direct observation on the face, which was too narrow to give a visible reflection.)

(773) One face of this form (new for pyrite) was observed on crystal 10. The face is quite narrow (0.1 mm.), but is sharp and gives a reasonably good reflection.

773 ∧ 111.

Measured	18° 28′
Calculated	18° 24′

(221) is a common form, and was observed on all the crystals measured,—

221 ∧ 111.

Measured	15° 54′	
Measured	15° 58′	(on various crystals;
Measured	15° 42′	signal generally good)
Measured	15° 46′	
	etc.	
Calculated	15° 47½′	

(This observation shows the degree of accuracy to be expected in the present work.)

(774), a new form for pyrite, was observed on crystal 3, with one narrow face between (221) and (111).

774 ∧ 111.

Measured	13° 05′	(signal rather good)
Calculated	13° 16′	

(553), observed on crystals 1 and 7.

553 ∧ 111.

Measured	12° 22′	(No. 1, signal poor)
Measured	12° 14′	(No. 7)
Calculated	12° 16′	

(332), observed on crystal 1. The zone containing it is somewhat rounded between (221) and (111), and gives by reflection a

continuous band. The two principal faces in the zone (332) and (553), however, give distinct, measurable reflections.

332 ∧ 111.

Measured	10° 00′	(signal poor)
Calculated	10° 1½′	

This close agreement, however, must be regarded as accidental.

(443), a new form for pyrite, was observed on crystals 6 and 10.

443 ∧ 111.

Measured	7° 28′	(No. 6)
Measured	7° 05′	(No. 10)
Calculated	7° 20′	

(554), a new form, was observed on crystals 1 and 6, with one face on each. The form is small but distinct.

554 ∧ 111.

Measured	5° 46′	(No. 1)
Calculated	5° 46′	

554 ∧ 221.

Measured	10° 2′	(No. 6, mean of two re-
Calculated	10° 1½′	flections at 9° 49′ and 10° 15′ from 221)

The agreement, curiously enough, is exact in each case, but this must be accidental.

(665), a new form, was observed as one small face on crystal 10.

665 ∧ 111.

Measured	4° 35′	(signal rather good)
Calculated	4° 45′	

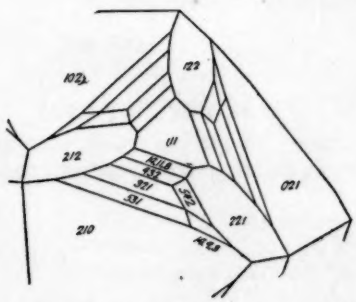


FIG. 3. Type I.

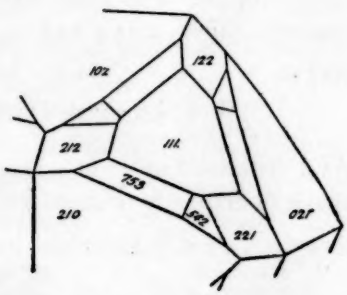


FIG. 4. Type I.

The diploids (421) (13.7.3) and (952) in the zone (112) appear as small, bright, triangular faces between (111) and (210), cutting

off the upper corner of the latter face. Considerable difficulty was experienced in measuring them, as they were brilliant only when quite small; the larger faces were poor and rounded.

(421) Only one face was observed; crystal 6 shows a small face whose intersection with (210) is sensibly parallel with the centre line of the latter. It is about 1 mm. long by 0.5 mm. wide.

421 ∧ 111.

Measured	28° 12′
Calculated	28° 6½′

(13.7.3) was found to be common. The form was determined from crystals 10 and 5.

13.7.3 ∧ 210.	ρ	ϕ	
Measured	8° 30′	11° 30′	(No. 10; signal good)
Measured	7° 55′	11° 49′	(No. 5; signal poor and divided into two. Angle given is mean position)
Calculated	8° 29′	11° 37′	

Other good measurements are

13.7.3 ∧ $2\bar{1}0$.	ρ	ϕ	
Measured	14° 05′	55° 40′	(No. 8; signal rather bright, but poor)
Calculated	13° 57′	55° 40′	

13.7.3 ∧ 100.	ρ	ϕ	
Measured	23° 00′	30° 22′	(No. 14)
Calculated	23° 12′	30° 22′	

Besides the above, (13.7.3) was observed on crystal 13.

(952) is a new form for pyrite. It affords smaller faces than (13.7.3) and while bright their smallness prevents any accuracy in measurement. The form was determined from two sets of measurements on crystals 4 and 14.

952 ∧ 210.	ρ	ϕ	
Measured	11° 40′	11° 15′	(No. 4; signal faint)
Calculated	12° 36′	11° 15′	

As a difference of 1° in ρ is equivalent to 12′ in actual position, the above discrepancy is about 11′.

952 ∧ 001.	ρ	ϕ	
Measured	28° 45′	79° 06′	(No. 14; signal poor)
Calculated	29° 03′	79° 12′	

(952) was also observed on crystals 5, 9, 10, and 12, giving measurements sufficiently good to identify the form after its occurrence had been established by the above.

The diploids (531), (321), (753), (432) and (14.11.8) in the zone ($1\bar{2}1$) between (210) and (111) were definitely observed on only two crystals, Nos. 11 and 15.

(531), observed on crystal 11 as one face about 0.5 mm. wide by 1.0 mm. long.

531 ∧ 111.

Measured	28° 38′ (signal good)
Calculated	28° 34′

(321), observed on the same crystal as a comparatively large face, 1 mm. by 1 mm.

321 ∧ 111.

Measured	22° 18′ (signal briliiant and
Calculated	22° 12½′ good)

(753), a new face for pyrite, was observed on crystal 15 only. The face is about 0.5 by 0.2 mm., and is quite good and bright.

753 ∧ 111.

Measured	18° 05′ (signal good)
Calculated	18° 5½′

(432) was definitely observed on crystal 11. It appears as a narrow face, somewhat rounded, about 0.1 mm. wide, being less than half the width of the face (14.11.8) occurring in the same zone.

432 ∧ 111.

Measured	15° 11′ (signal rather bright but
Calculated	15° 13½′ poor)

(14.11.8) appears as a good, brilliant face about 0.3 mm. wide, on crystal 11.

14.11.8 ∧ 111.

Measured	12° 32′ (signal bright and fairly
Calculated	12° 32′ good)

(14.9.3), a new form for pyrite, occurs as a good face about 0.5 mm. wide on crystal 11. Its symbol was determined from measurement to two adjacent faces, as follows,—

	14.9.3 ∧ 111.	14.9.3 ∧ 221.	
Measured	15° 01′	27° 28′	(signal bright and good)
Calculated	15° 01′	27° 26′	

(542), a new form for pyrite, was observed on crystals 6, 11, and 15. In each case the face was small but bright.

542 ∧ 111.

	ρ	ϕ	
Measured	33° 55′	21° 16′	(No. 6; signal faint but
Calculated	33° 57′	21° 03′	good)

(In the zone or (221) and (321).)

542∧221.

Measured	6°	(No. 11; signal good)
Measured	6° 13′	(No. 15)
Calculated	6° 24′	

The faces observed on each crystal are set forth in the following table:

Form.	Crystal No.														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
100		*								*			*	*	*
111	*	*	*	*	*	*	*	*	*	*	*	*	*	*	*
210	*	*	*	*	*	*	*	*	*	*	*	*	*	*	*
331		+													
11.11.4							+								
552						*	+			+					
773										+					
221	*	*	*	*	*	*	*	*	*	*	*	*	*	*	*
774			+												
553	+						+								
332	+														
443						+				+					
554	+					+									
665										+					
531											+				
321		?									+				
753															+
432		?									+				
14.11.8											+				
421			+												
13.7.3					+			+		+			*	+	
952				+	*				*	*		*		+	
14.9.3											+				
542						+					+				+

(* indicates that the measurements were used to determine the form and are given above under the description of faces.)

A detailed description of the fifteen crystals measured is unnecessary, but several crystals will be taken up in detail to give an idea of the relative prominence of the observed forms.

Crystal 4 is a typical crystal illustrating (952). It is a fragment, less than half of a complete crystal, about 4 mm. long. It shows one end of a cubic axis, with parts of four octahedron faces and two (210) faces, the larger of the latter being 3 mm. by 1.5 mm. and the other about half that size. One trisoctahedron face is present; if the larger pyritohedron is called (102) this trisoctahedron will be (122). It is about 0.5 mm. wide. The face (529) appears cutting off the corner at the junction of (102), (122) and (111). It is 0.5 mm. by 0.1 mm. and very perfect and bright.

Crystal 10 may be taken as typical of those showing the various trisoctahedrons well developed. It is a fragment about 5 mm. in diameter. It shows two complete pyritohedron faces each about 2 mm. wide. If we put them in the position of (102) and ($\bar{1}02$) respectively, the other faces developed will be these,—about half of (021) and a small part of (021). The zone ($\bar{2}10$), ($\bar{1}00$) and ($\bar{2}\bar{1}0$) appears, showing a small fragment of these faces. This is the only appearance of the cube on the crystal. The octahedron appears as small portions of the four upper faces. Three trisoctahedron zones appear, (221) being most prominent and giving faces about 1 mm. wide.

One of these zones shows two small faces occupying the position of (552) or (331); they are too minute to give a reflection. The next zone is rich in faces. Starting with the octahedron having the position (1$\bar{1}$1), we have in order, (1$\bar{1}$1), (5$\bar{6}$6), (1$\bar{2}$2), (3$\bar{7}$7) on one side, and on the other ($\bar{2}\bar{5}5$), ($\bar{1}\bar{2}2$), ($\bar{1}\bar{1}1$). The two (221) faces are about 0.5 mm. wide; the other trisoctahedrons uniformly about 0.1 mm. The third zone is similar, but shows two faces of (552) instead of one of (552) and one of (773), and a face of (443) instead of (665).

Two diploid faces are present, in the positions (529) and (7.$\bar{3}$.13). The face of (952) is extremely small and bright, and cuts off the corner formed by (102), (111), and (122). The face of (13.7.3) is larger,—1 mm. long by 0.2 mm. wide,—and rounds off into the adjacent octahedron face, the rounded part being a conical

surface. This rounding off was often noticed on (13.7.3) but not on (952).

Crystal 11 shows a rare combination of forms. It diverges remarkably from the type, and indeed might have been supposed to come from another locality, were it not for the perfectly typical markings on the pyritohedron and octahedron. Again, one of the forms it shows, (542), is found on crystals 6 and 15, which are perfectly typical in all respects. It is a rough fragment 1.5 cm. long, showing on one end an octahedron face with two adjacent (221) faces, and one face each of (321), (542), (14.11.8), (432), (14.7.3). A rough curved, irregular surface breaks the (111, 210) zone beyond (321) for the space of 2 mm.; (521) and (210) appear beyond this irregular portion. The octahedron and (221) faces are each a couple of mm. in extent; the face of (321) is 1 mm. each way, and

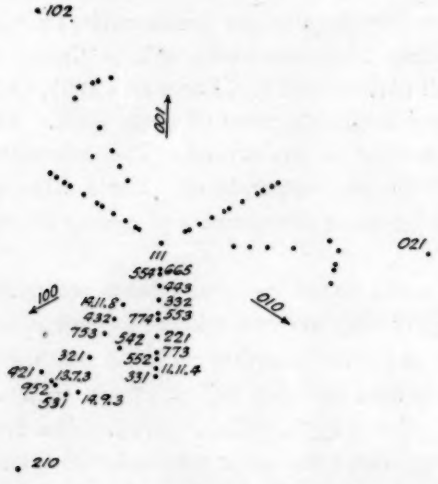


FIG. 5. Type I. Gnomonic projection on plane of octahedron. Illustrating zonal relations.

the (14.7.3) and (531) faces are 0.5 mm. wide; (14.11.8) and (542) are about 0.3 mm. wide, and (432) about 0.1 mm. If the pyritohedron be regarded as (102), the observed faces have the positions, (102), (1$\bar{1}$1), (2$\bar{1}$2), (1$\bar{2}$2), (3$\bar{1}$5), (2$\bar{1}$3), (3$\bar{2}$4), (11.$\bar{8}$.14), (4$\bar{2}$5), and (9.$\bar{3}$.14). Fig. 3 shows the combination of forms observed on this crystal.

Crystal 15 shows two octahedron faces, (111) and (1$\bar{1}$1), and a portion of (102). One face of the trisoctahedron is present, in the position (212). It is a trifle less than 1 mm. wide. (537) lies in the zone of (111) and (102); it is about 0.5 mm. by 0.2 mm. in extent. (425) appears as a nearly equilateral triangle about 0.2 mm. on a side between (212), (537) and (1$\bar{1}$1). A portion of (210) is present below (212). (425) lies in the zone (210, 212). Fig. 4 shows this combination of forms.

Zonal Relations of Type I.

The zone of trisoctahedrons, symbol (1$\bar{1}$0), seems to be the most important in this type. It affords the greatest number of forms, and determines the growth figures on the pyritohedron face and the direction of one set of striations on the octahedrons.

The zone of (111) and (210), symbol (1$\bar{2}$1), which is usually well developed in pyrite, seems to be of secondary importance in this type. While it determines the second set of octahedral striations, it was only observed on three crystals,—2, 11, and 15. On the last two of these, however, it affords a good series of forms, (531), (321), (753), (432), and (14.11.8).

The zone ($\bar{1}$12) is important, affording (421), (13.7.3), (952) and (531), but curiously enough only a single face of this zone appears at a time. The primary importance of the zone of trisoctahedrons again appears in the fact that each of the above faces lies between (210) and a trisoctahedron of simple ratio, thus,—

210	210	210		210
421	13.7.3	952	and	531
212	323	434		111

This is also true of the scattering diploids (14.9.3) and (542).

210	210
14.9.3	542
443	332

These trisoctahedrons have the same ratio as the ones above, but are in a different zone.

The occurrence of the zone of trisoctahedrons as an important zone is so rare in pyrite that the writer has been moved to call this

type the "trisoctahedral type." While a number of trisoctahedrons have been described on pyrite, their occurrence is usually scattering, one or two from each locality, and not in a complete series, as in the present case.

It will be noticed that the general shape of the crystal as indicated by Fig. 1 approaches a sphere, pointing to growth under great pressure.

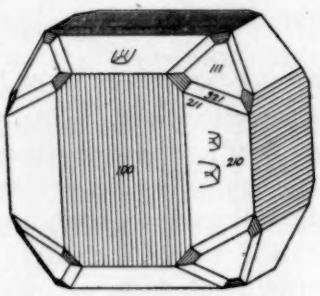


FIG. 6. Type II.

TYPE II.

This type affords larger and more perfect crystals, which often attain a diameter of 2 or 3 cm. In habit the crystal is a combination of cube and pyritohedron (210) with the octahedron, the diploid (321) and the trapezohedron (211) commonly occurring. The cube is striated in the usual manner for pyrite, parallel to the intersection with (210), and the (211) face is striated parallel to its intersection with the octahedron. Large natural etching figures are common on the cube, approximately square, in diagonal position. There is reason to believe that the striation on (211) is due to natural etching and not to growth. The pyritohedron has growth figures as shown in Fig. 6. With the exception of the cube and trapezohedron the faces are in general fresh and lustrous.

A series of seven crystals was measured, and the following forms observed. Those marked (*) are new for pyrite. In addition, two forms (15.11.7) and (11.8.5) have been previously observed from but one locality,—Porkura.[1]

[1] B. Mauritz in Groth's *Zeitschrift*, Vol. 39 (1903), p. 357.

	Diploids.	Trisoctahedrons.	Trapezohedrons.
110	321	221	211
210	*753	*774	*744
111	11.8.5	*552	
	15.11.7		
	14.11.8		
	*876		
	*12.11.10		

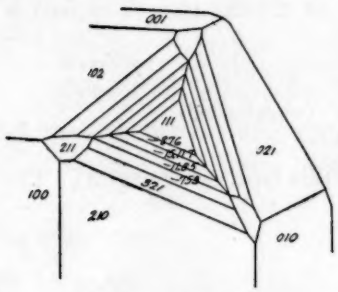


FIG. 7. Type II.

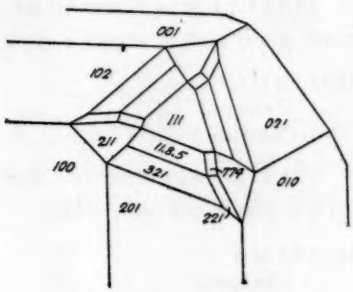


FIG. 8. Type II.

The observations determining these forms are as follows:

(321) was a face commonly observed. Some of the measurements are:

321 ∧ 111.

Measured	22° 14′	(Crystal 22)
Measured	22° 13′	(Crystal 22)
Measured	22° 10′	
	etc.	
Calculated	22° 12½′	

showing the degree of accuracy that may be expected in this type.

(753), a new form, was observed on crystals 23, 26 and 27,—

753 ∧ 111.

Measured	18° 05′	(Crystal 23)
Measured	17° 58′	(Crystal 26)
Calculated	18° 05½′	

753 ∧ 321.

Measured	4° 05′	(Crystal 27)
Calculated	4° 07′	

It is almost unnecessary to state that the above, as well as all other diploids observed on this type, were found to be sensibly in the zone of (111) and (210), so that one angle from the octahedron determines them.

(11.8.5) was observed as one narrow face on crystal 21, and two fairly good faces on crystal 23.

11.8.5 ∧ 111.

Measured	17° 05′	(Crystal 21; signal poor and faint)
Measured	16° 59′	(Crystal 23; signal good)
Calculated	17° 01½′	

(15.11.7) was observed as one face on crystal 25. The face is quite good and is about 1 mm. wide.

15.11.7 ∧ 111.

Measured	16° 33′	(signal good)
Calculated	16° 32′	

(14.11.8) was observed once only on this type, on crystal 26. The face is about 0.2 mm. wide.

14.11.8 ∧ 111.

Measured	12° 37′
Calculated	12° 33′

(876) was observed as one good face about 0.7 mm. wide, on crystal 23.

876 ∧ 111.

Measured	6° 44′
Calculated	6° 39′

(12.11.10), a new form, may almost be regarded as vicinal. On crystal 26 the following measurement was obtained:

12.11.10 ∧ 111.

Measured	4° 19′	(signal poor)
Calculated	4° 15′	

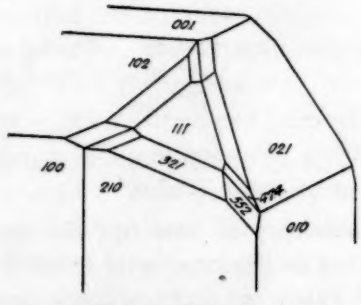


FIG. 9. Type II.

(221) and **(774)** were observed on crystal 23. They were extremely narrow faces, each lying in a zone with (211) and a diploid. Their narrowness prevented their giving any definite signal, but they were very perfect and gave fairly good results by direct observation, that is with the small auxiliary lens down in front of the telescope objective. There was no doubt as to the diploid determining (211). It gave,—

321 ∧ 111.

Measured	22° 13′
Calculated	22° 12½′

but the one determining (774), that is (11.8.5), was not definite. This problem was solved by measuring the angle between the zones (211)(774) and (211)(111) on a petrographical microscope reading angles to 5′.

121 ∧ 221.

Measured	11° 36′
Calculated	11° 30′

121 ∧ 774.

	ρ	ϕ
Measured	43° 39′	17° 01′
Calculated	44° 01′	17° 05′

The remaining trisoctahedron (552) will be discussed along with the trapezohedron (744) for a special reason.

(211) is of common occurrence and cannot be mistaken, on account of its striation. It usually gives a poor reflection. A measurement on crystal 22, however, gives,—

211 ∧ 111.

Measured	19° 28′
Calculated	19° 28′

(744) and **(552)** were observed on crystal 24. The pyritohedron (210) and the octahedron are present, and two faces of (321), one of which lies in a zone with (210) and (111). The two faces considered lie in a zone with (210) and the other diploid face (132). They are both extremely narrow, (744) not being wide enough to afford a satisfactory signal, and requiring measurement by direct observation.

	210 ∧ 474.	210 ∧ 552.
Measured	42° 23′	23° 48′
Calculated	41° 49′	24° 06′

There is considerable discrepancy here, (474) in particular being half a degree out. This is very probably due to the inaccuracy of the method by which it was measured. It is much more probable that the faces have the symbol given than that they are diploids with complex ratios approaching these symbols, for two other trisoctahedrons have been observed, (221) and (774), and one other trapezohedron (211), while no diploids were observed except in the zone of (111, 210).

It will be noticed that one form given by Blake, namely (432), was not observed,—a form with simple ratio, and one which if it occurred at all would be more common than forms of more complex ratios, as (15.11.7) and (11.8.5) There is some doubt in the writer's mind as to whether the (432) of Blake really is (432) or is a diploid with more complex ratio, as the above.

The following table sets forth the forms observed on each crystal.

Form.	Crystal No.						
	21	22	23	24	25	26	27
100	*	*	*	*	*	*	*
210	*	*	*	*	*	*	*
111	*	*	*	*	*	*	*
321	*	*	*	*	*	*	*
753			*			*	*
11.8.5	*		*				
15.11.7					*		
14.11.8						*	
876			*				
12.11.10						*	
221			*				
774			*				
552				*			
211	*	*	*		*	*	*
744				*			

Crystal 23 is perhaps the only one of this series worthy of a separate description. This crystal is about 1.8 cm. in diameter. It

shows the cube (100) with its adjacent pyritohedron faces (210) and (2$\bar{1}$0), and (001) with (102). There are two octahedron faces, (111) and (1$\bar{1}$1). The former face, which is surrounded by the zones containing the new diploids, would have been 3 mm. on a side, but another smaller crystal has grown into it, leaving only two corners. Of the three zones of diploids that surround (111), one has two faces in it, (11.8.5) and (321); the second three, (876) (11.8.5); and the third two, (753) and (321). The (876) and (753) faces are about 0.7 mm. wide, and the (11.8.5) face in the second zone is 1 mm. wide. All are perfect faces. The (11.8.5) zone is good at one end, but at the other runs off into a broken vicinal face. At its good end is a face of (211), about 1 mm. long. Between this latter and the (11.8.5) and (321) faces lie the trisoctahedrons (221) and (774) described above.

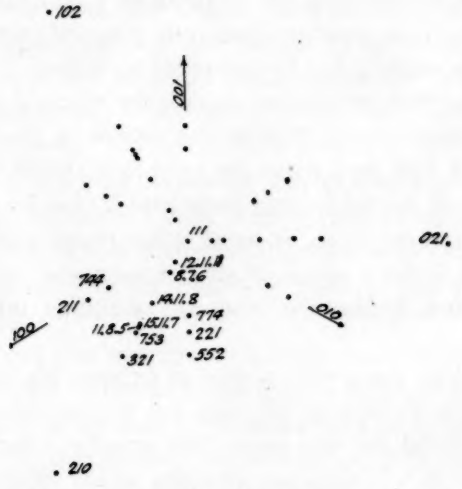


FIG. 10. Type II. Gnomonic projection on plane of octahedron. Illustrating zonal relations.

ZONAL RELATIONS OF TYPE II.

The most important zone in this type, outside of the cube-pyritohedron zone, is that of the octahedron and pyritohedron, symbol (1$\bar{2}$1). This affords all the observed diploids, seven in number, one of which (321) is practically always present. The trisocta-

hedron zone was developed on one crystal, affording (221) and (774), each of which was in a zone with (211) and an observed diploid. Thus,—

211	211
212	747
213	8.5.11

This is not true of (552), which would agree with the diploid (741). No diploids were observed between (321) and (210), (321) being the last of the series.

The development of the zone between the pyritohedron and a non-adjacent (321) face, e. g., (210) (132) on crystal 24 is probably accidental, due to the irregular development of the crystal. This zone affords (552) and (474).

The writer wishes to call attention to the method described above, used in determining the small faces (221) and (774). He has not heard of its previous use as a practical method of measurement. The angle ρ can be measured to within 5′ or 10′ with a microscope, and this is accurate enough for all purposes if ϕ is not large. The easiest way of holding the crystal on the stage is to set it on a cone of wax on a glass slip; the face chosen as a reference face can be made approximately horizontal by hand. Having measured ρ, the crystal can be set up on an ordinary one-circle goniometer, and, using the Websky signal, ϕ may be measured by observing the face directly and finding the point of maximum brilliancy of reflection.

In concluding, the writer wishes to express his indebtedness to Professor Amos P. Brown, not only for the specimens which furnished the material for this paper, but also for valuable assistance in its preparation. Thanks are also due to Mr. Kenneth Williams for crystals of Type II used in the above series.

ELEMENTARY SPECIES IN AGRICULTURE.

By PROFESSOR HUGO DE VRIES,
AMSTERDAM.

(*Read April 18, 1906.*)

Franklin's name is honored all over the world, and the splendid services he has rendered to science and to humanity have had their influence in Europe as they have in America. Your president and secretary, in inviting me in your name to attend this celebration of the bicentennial of your great founder's birth, have offered me a welcome occasion of paying a tribute to his memory. It is not, however, without hesitation that I have accepted this honorable invitation. Philadelphia has been the center of botanical interest and research in this country for more than a century. The various contributions of your society to biological science are followed in Europe with intense interest. To speak before such a famous sphere of learning is not only a high honor, but also imposes a great obligation. In accepting the invitation I have trusted to your indulgence and to the interest shown by you in the broad questions of evolution, which of late have returned to the empirical methods and principles laid down by Darwin.

New facts and new conceptions are the result of half a century of industrious work. Darwin relied for a large part on the methods of selection which at his time were in use both in agriculture and in horticulture. He tried to show that the evolution of species at large has followed the same laws that underlie the evolution of races and varieties in culture. In broad lines he has succeeded in convincing his contemporaries of the validity of this analogy. Agricultural and horticultural experience, however, were at his time only imperfectly developed, and the amelioration of races, though successful in a large number of cases, had no really scientific basis. It did not afford all the evidence required for a thoroughly reliable theory. Complying with the prevailing belief of the most renowned agriculturists, which

considered the breeding of races as a slow process of gradual amelioration, he proposed the same slow and almost imperceptible changes as the source of evolution in nature. Since his time experience and theory have made very manifest advances. Especially the principle of the unit-characters, which is the basis of the theory of origin of species by mutation, leads us to the acceptation of saltatory changes or so-called sports as the most probable way of nature to produce new forms. According to this theory species are not changed into one another, but new forms arise sideways from the old ones. The whole strain continues unchanged and only produces from time to time single aberrant individuals. These are the real sources of all progress, and experience has shown that in the main their new characters are hereditary, and that their progeny remains true to their new types even from its first appearance.

In agricultural breeding-practice the production of new races is an intricate problem. In many cases their relation to the theoretical conceptions is quite clear, in others it is still surrounded with doubt. In my book on the mutation theory I have explained how the obvious facts agree with that idea, but it was at that moment impossible to remove all doubts and so I purposed to return to these questions another time (Mut. Th., I, p. 82). Five years have since elapsed far more complete analysis of the agricultural breeding processes. and new discoveries have been published which enable us to give a Especially at the agricultural experiment station in southern Sweden quite unsuspected facts relating to the variability of agricultural crops have been brought to light. They are of a nature to throw over all the old ideas concerning race amelioration and give proof that the methods now generally in use in Europe are faulty as well from a practical as from a scientific point of view. The director of that station, Dr. Hjalmar Nilsson, has discovered that most of our ordinary agricultural crops are not only composed of elementary species, as was long known before him, but that each cultural variety contains hundreds of sharply definite types. These are widely distinct from one another as well in botanical characters as in those properties which decide on their utility from the breeder's point of view. Moreover, they differ so widely from one another

as to respond to almost all the requirements of the agricultural practice. By simply searching among them, the proper type may forthwith be found for almost each gap in practice. In this way they are seen to afford almost unexhaustible material for selection.

For to-day's theme I have chosen an application of these discoveries of Nilsson to a criticism of the current views concerning the bearing of agricultural breeding processes on the theory of evolution. Formerly I gave the warning not to trust too much to these processes and to make use, in scientific discussions, only of the most simple and clear cases (Mut. Theory, I, p. 59). The new facts, now at hand, go to prove that even the apparently simple methods of selection have been far more complicated than their authors suspected. The slow and gradual working up of a cereal to a previously fixed ideal seemed to be a process of the simplest possible nature. In reality, however, it is composed of a series of factors, which the breeders themselves have not recognized, and which, therefore, it is now often impossible to discern in their descriptions. In broad lines such an analysis has been made practicable by Nilsson's discoveries. Unfortunately it conduces to a less high appreciation of the breeder's merits (Mut. Th., p. 82), but on the other hand it gives a stronger support to the theory of the saltatory origin of species.

The breeding of cereals results in varieties, which are as constant and independent as the best horticultural sorts. In some cases they are known to originate in the same way, by accidental sports, as in the instance of Beseler's oats losing their needles. Here their complying with the principle of mutation is obvious. In the large majority of cases, however, including the most renowned improvements of cereals and other crops, they are said to have been produced by the common slow and gradual process of selection. All such cases are surrounded with doubt, as well concerning their real origin, as in view of the degree of self-dependency which is reached at the end. Often practical reasons lead one to prefer the original seed to one's own harvest, especially when it is difficult to keep the cultures clean from vicinistic impurities. A race, which is really self-dependent, may in this way seem to be permanently related to the

continuous selection of its pedigree. It is especially in Germany that this method of slow amelioration is much beloved and has given admirable results. One of the best known instances, and for which the historical records are the most complete, is the renowned rye of Schlanstedt, produced by Rimpau, which is now largely cultivated all over the central parts of Germany and the northern districts of France. In the year 1876 I had the privilege of visiting Mr. Rimpau on his farm at Schlanstedt and of studying his cultures. The choicest of his new rye occupied a small patch out on the fields, but surrounded by cultures of vegetables and other plants not belonging to the cereals. These minor cultures occupied a large square, which in its turn was surrounded by a complete range of shrubs. Thus the rye, standing in the midst of the square, was suffi-possible contamination by pollen of other varieties. On the other hand, it was given the same soil and exposure and almost the same cultural treatment as the average cultures.

This race had been started by Rimpau nine years before, in the year 1867. At the time of the harvest of that year he had inspected, as he told me, a large number of his rye fields and selected all the ears which seemed to him to surpass the others quite strikingly. He brought home a handful of them, repeated the trial and mixed their seeds. This mixed condition of his seed in the beginning of his race has now become the weak point, where the whole principle of his method is open to criticism.

The seeds were sown next year, and in the harvest the same selection of the best ears was repeated. Care was taken to exclude all those, which by some external condition could have profited from more space or more manure than the remainder, and could have grown large by such accidental means. No care, however, was taken to isolate the individuals and to sow their seeds separately, the principle being that all the plants belonged to one race, and that this race had to be ameliorated. This principle of ameliorating a race without isolating its possible constituents seemed at that period to be the right one, though now it can hardly be considered as scientifically correct.

Each year in the same way the best ears were chosen from the continuance of the choicest strain, and after the exclusion of all ears

of minor value the remainder were sown on a field and multiplied without further selection in order to produce all the seed required for the sowing of the whole farm. It took three or four years to reach this quantity. After twenty years of continued selection the choice strain was so much improved as to produce a race distinctly richer than the ordinary varieties of rye in middle Germany, and slowly but gradually it found its way first into the surrounding farms and afterwards over large parts of the country. During this period Rimpau was enabled to sell all his harvest as seed-grain, obtaining in this way a most satisfactory recompense for his labors. Shortly afterwards the rye of Schlanstedt was introduced into France, where it soon overthrew the local varieties, especially in the departments north of Paris. Even there it is ordinarily cultivated from original seed, produced directly by Rimpau or multiplied only during some few generations by seed merchants.

For our critical purpose, it is highly interesting to note how a French agriculturist, Professor Schribaux of the Institut Agronomique of Paris explains the conditions of keeping the Schlandstedt rye up to its original qualities. He says: "In order to do this, care must be taken to sow the seeds on a field which is as far removed as possible from all other cultures of rye. Moreover, the field should be large and protected all around by a hedge of trees and shrubs. Without this precaution the rye of Schlanstedt would soon degenerate through accidental crosses with the local varieties." Such crosses would, under any other conditions, be unavoidable and soon wholly deteriorate the race ("Almanach du Cultivateur," 1892, p. 69).

From this judgment, given by an authority who has so much contributed to the wealth of northern France by the introduction of this variety, we may deduce some conclusions as to the constancy of Rimpau's rye. It is clear that Schribaux takes the race to be substantially constant, and explains the necessity of continued selection only by the impending danger of crosses with varieties of minor value. Hence it follows that the main significance of the pedigree culture on the farm of Rimpau must be the same, and that at least in later

years his pedigree must have gained a degree of uniformity, which is in no need of any further improvement. The real act of effective selection is thereby brought back to the first years, but how many generations of true selection it has taken to render the rye of Schlanstedt uniform and pure it will, of course, always remain impossible to tell. The explanation of Rimpau's success must, therefore, for a large part remain hypothetical. If now we try to give such an explication on the ground of the theory of mutation and of the already quoted discoveries of Nilsson we may suggest the following: At the period when Rimpau started his pedigree, his rye fields must have contained numerous elementary species, not observed or distinguished by him or by any other agriculturist of his time. Among the ears which he selected a good number of these aberrant types will, of course, have been represented, since he selected only those which caught his eye by some striking useful difference from the main type. Of course, he sought for ears of one and the same ideal type, having a large number of big kernels. But notwithstanding this, his handful of ears must have belonged to more than one elementary species. Among these units of his selection some will have been better yielders than others, and the subsequent selection of his twenty years of pedigree-culture will slowly but surely have eliminated the units of minor worth. This would result, at the end, in a complete isolation of the best one of all the types, which he originally, but unconsciously, selected and mixed.

Or, in other words, Rimpau's pedigree culture was started as a mixture of a number of excellent types, and his yearly selection gradually reduced this number, until he had isolated and purified the very best one among them. This point was, of course, only unconsciously reached, but then it must have made his rye independent of all further real selection, reducing the process to the care of excluding vicinism.

If this explication of Rimpau's process is true it, of course, holds good for all similar cases of slow and gradual amelioration of agricultural plants by selection. Thereby it would deprive the theory of the origin of species by small and continuous changes of its last support in the realm of the vegetable kingdom.

It remains to be shown that the new facts give sufficient proof of the exactness of this suggestion. They relate to the question of the part which fluctuating variability and mutability may have played in the selection culture of Rimpau. An exact notion of the first phenomenon, as stated by the works of Quetelet (1870) and Galton (1889) had only found its way into botanical investigations about the year 1894, or nearly twenty-five years after Rimpau started his pedigree of rye. At his time, therefore, no distinction of this kind could be made, and it is only natural that he took his selected specimens to be the extremes of ordinary fluctuating variability

This point of view and this lack of distinction between the now so clearly contrasting processes has prevailed for a long time among agriculturists. As an instance I may quote the work of Willet M. Hays, now in Washington (1899, Bull. No. 62, Agric. Exp. Station, Minnesota). He has ameliorated the wheat of Minnesota by breeding, from the local races Fife and Blue Stem, better and more yielding varieties, which now in large part have supplanted the old types. Besides his practical results he has given some theoretical considerations in which he compares his selected mother plants with the principle of fluctuating variability and explains them as extremes in the curves which constitute the law of Quetelet. " In each one thousand plants of wheat," he says, " there are a few phenomenal yielders, and the method of single-seed planting makes it practicable to secure these exceptional plants, and from these new varieties can be made " (p. 429). But according to our present knowledge, the isolation of such plants, if they were truly extremes of fluctuating variability, would lead to a regression to mediocrity, as it has been called by Galton, and not to constancy nor to an exact keeping up of the extreme type. Therefore the supposition is allowed that the phenomenal yielders of Hays, were in reality representatives of distinct elementary species, which had been hidden until his time. His method of selecting enabled him to single them out, and his new principle of single-seed planting, which conduced to his high achievements, at the same time indicated the way for an explication on the basis of our present views concerning the different types of variability.

It would take me too long to describe the methods and cultures of the Minnesota Experiment Station, and I may assume that their leading principles and practical results are well known. But I wish to point out that, exactly in the principle of sowing the seeds of individual selected plants separately, Hays gained a distinct advantage over the slow process of Rimpau and the other German breeders. He found, by his method, that the isolated strains are at once constant and pure. They had only to be multiplied in order to give a new race. Of course, the different mother plants had to be compared in their progeny, and among a large number of such new pedigree races only one or two were found to be the very best. The remainder had to be rejected, and only those few excelling ones could be introduced with advantage into the field-cultures of the state.

If now we compare this principle of Nilsson and Hays with the method of Rimpau we find that the Swedish and American breeders by one single choice isolated the very best strains and observed them to be constant and pure. The German breeders, on the other hand, by selecting a number of ears, must have got impure races, and wanted a long succession of years and a constantly repeated selection in order to reach the same result in the end.

Hence we may deduce the supposition that if Rimpau in starting his experiments, now forty years ago, had had at his disposition our present knowledge of variability, he would have sown the kernels of his selected ears separately and selected at once among the resulting strains the very one which now bears the name of his farm. No continuous culture and repeated selection would have been needed, and the false appearance of a slow and gradual improvement of a race by selection would simply have been avoided.

The German breeding process has always been one of the most valuable arguments for the theory of gradual selection and was of late considered as its last botanical support. By means of the discoveries of Nilsson and Hays this support has now been broken down, and agricultural selection is no longer an argument against the conception of an origin of species by saltatory changes.

DILUTE SULPHURIC ACID AS A FUNGICIDE.

By HENRY KRAEMER.

(*Read April 20, 1906.*)

Sulphur has been used as a disinfectant, or deodorant, since very ancient times. In The Odyssey reference is made to the burning of sulphur for this purpose. As a parasiticide, insecticide and fungicide it is extensively used at the present time.

The value of sulphur as a fungicide in destroying epiphytic fungi, particularly the powdery mildews of the rose, vine, hop, etc., is well known. Sulphur is also sometimes used in destroying the spores of endophytic fungi, or at least in preventing their growth. As a fungicide sulphur is used or applied in several ways. (1) In the sublimed form (flowers of sulphur) it is dusted on the plants. (2) Both sublimed sulphur and ground roll-sulphur, or "brimstone" (flour of sulphur), are mixed with water to form a paste, which is applied to the heating pipes in green-houses, the finely divided vaporized sulphur thus produced collecting on the plants; or small pieces of roll-sulphur may be carefully heated on a sand bath, the finely divided vaporized sulphur being distributed in the same way; or sulphur in the form of an impalpable powder known as "ventilated sulphur" may be used, as in Germany and Italy where it is used for fighting the vine blight. (3) Sulphur is also applied in combination with lime in the form of solution, as in the "lime-sulphur" washes, these being solutions of a number of sulphides and lower sulphates of calcium.

Sulphur is insoluble in water and is not affected by most reagents under ordinary conditions, and of itself could not be considered to have fungicidal properties. Nor is it likely that it exerts a mechanical action like certain of the insecticides, as pyrethrum flowers, or insect powder, which act by closing the breathing pores of the insects. The action of sulphur as a fungicide is probably due, then, to certain compounds of sulphur which result from the manner of

using it. It is generally considered by plant pathologists that when sulphur is used in a powdered form as a remedy for plant diseases, the effects are due to sulphur dioxide. While sulphur dioxide undoubtedly does have a powerful fungicidal action, it is usually conceded that sulphur dioxide and sulphurous acid are exceedingly toxic to higher plants as well. Owing to the high temperature at which sulphur inflames (260° C.) and the rapid evolution of sulphur dioxide, as well as its unequal diffusion, sulphur is never burned in order to secure its fungicidal effects, but is employed either in the form of a powder, or by gently heating it, when a certain portion of it is sublimed, thus distributing it over the plants in a finely divided state. In Italy the grape-growers and horticulturists have found that the efficiency of powdered sulphur when dusted on plants is in direct ratio to the degree of fineness.

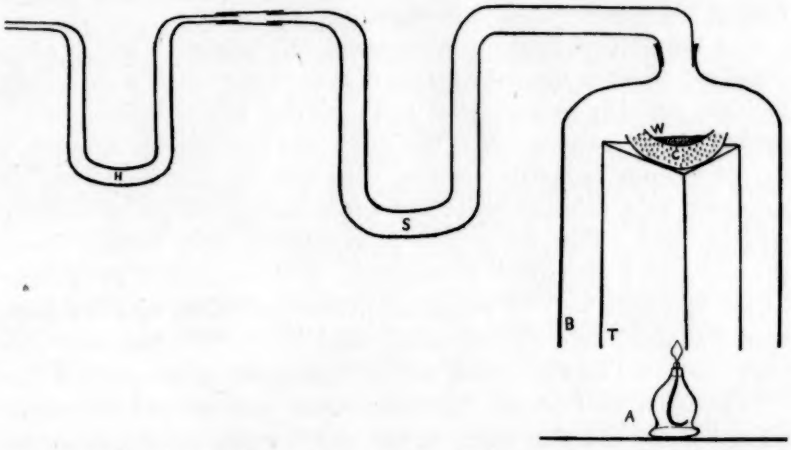
It is stated by Bloxam[1] that "finely divided sulphur, especially sublimed sulphur, is gradually oxidized and converted into sulphuric acid when exposed to moist air." It is also well known that sublimed sulphur contains a certain amount of sulphuric acid. Not only is this true, but it is claimed that if the sublimed sulphur be not dried after washing it to free it of acid, sulphuric acid is again formed. Furthermore, it has been shown by Pollacci[2] that, independent of the presence of organic matter, sulphur is changed directly into sulphuric acid when mixed with the soil.

The fact that sulphur is easily oxidized in a moist atmosphere to sulphuric acid taken together with the fact that sulphur when vaporized on steam pipes is more efficient as a fungicide than when the flowers, or flour, of sulphur are dusted on the plants, led to the supposition that this increased efficiency might be due to the increased proportion of sulphuric acid formed. Accordingly, experiments were undertaken to determine what compounds are formed when sulphur is slowly heated. An apparatus was constructed for heating sulphur and for collecting the gases formed. The apparatus for collecting the gases consists of a bell jar connected with two U-tubes. The bell jar is suspended over a plate in such a manner as to permit of the free entrance of air from below, or at a distance of 5 to 10 cm.

[1] Bloxam's Chemistry, seventh edition, 1890, p. 189.

[2] Pollacci, *Gaz. chim. ital.*, through *Jour. Pharm. Chim.*, 1874, p. 330.

above the plate. The sand bath used for heating the sulphur is placed on a glass tripod under the bell jar, and an alcohol lamp is used to heat the sand. A definite quantity of N/10 sodium carbonate solution is placed in the U-tubes and the free end connected with an aspirator, or suction pump.



Apparatus for heating sulphur and collecting acid gases formed. *A*, alcohol lamp; *T*, glass tripod; *C*, porcelain dish containing sand; *W*, watch crystal containing sulphur; *B*, bell jar supported by a clamp; *S*, U-tube with one end ground to fit bell jar and containing 15 c.c. of tenth normal sodium carbonate solution; *H*, U-tube containing distilled water, and having one end connected with U-tube, S, by means of rubber tubing, and the other with an aspirator.

The operation was conducted as follows: To the first U-tube, which was well ground to fit the opening in the bell jar, were added 15 c.c. of tenth normal sodium carbonate solution to combine with the sulphur acids formed. To the second U-tube 5 c.c. of water were added. Definite quantities of sulphur were weighed out in a watch crystal, and heated on the sand bath for several hours. The heat was regulated to a certain extent so as to minimize the loss of the vaporized products due to their escaping from below the bell jar, that is, the flame was removed at times and the space between the plate and bell jar was reduced to but a few centimeters. At the end of the operation the apparatus was allowed to cool, and the watch crystal containing the sulphur weighed to determine the total

loss of sulphur by vaporization. The contents of the two U-tubes were then poured into a graduate and the U-tubes, the bell jar and the plate carefully rinsed with distilled water and the rinsings added to the graduate, the contents of which was made up to about 200 or 300 c.c. One-half the quantity of solution was used for determining the sulphuric acid formed, while the other half was used for determining the sulphurous acid present.

To the portion used for determining the sulphuric acid 5 additional c.c. of tenth normal solution of sodium carbonate were added, and the solution titrated with tenth normal sulphuric acid, using methyl orange as indicator. The difference between the number of c.c. of sodium carbonate solution used and the number of c.c. of sulphuric acid solution consumed gives the equivalent of sulphur acids formed. That the greater proportion of acids formed under the conditions described is sulphuric acid, is shown by adding barium chloride with an excess of hydrochloric acid to a hot solution, the precipitate formed being insoluble. The amount of sulphur converted into sulphuric acid is very easily calculated.

The other half of the original solution was titrated with decinormal iodine solution using starch test solution as indicator, but it was found that the proportion of sulphurous acid formed by heating or subliming the sulphur in the manner indicated was quite small.

In the following table the results of a number of experiments are given:

	Weight of Sulphur Heated.	Number of Hours Heated.	Loss of Sulphur by Vaporization.	Percentage of Vaporized Sulphur Converted Into Sulphuric Acid.	Percentage of Vaporized Sulphur Converted Into Sulphurous Acid.	Percentage of Vaporized Sulphur Driven Off as Sublimed Sulphur.	Remarks.
1	0.200	3	0.050	8.02	0.31	91.67	
2	0.500	3	0.120	7.08	0.53	92.39	
3	0.500	3	0.110	7.57	0.28	92.64	
4	0.500	3	0.120	9.53	0.10	91.37	
5	0.500	3	0.128	8.72	0.24	91.04	
6	0.500	3	0.150	13.64	0.31	86.05	
7	0.500	5	0.200	7.08	0.15	92.77	
8	1.000	7	0.030	25.34	0.26	73.40	Very slowly heated.
9	1.000	8	0.035	28.03	none	71.97	Very slowly heated.
10	2.000	3	0.880	6.90	0.25	92.85	Rapidly heated.
11	1.000	3	0.975	3.76	0.29	95.95	Rapidly heated.
12	3.000	3	1.085	1.68	1.38	96.94	Rapidly heated.

The table shows that when sulphur is heated very slowly with free access of air a large percentage (nearly one-third) of the vaporized sulphur is converted into sulphuric acid, very little or no sulphurous acid being formed. When the temperature is increased and the sulphur more rapidly vaporized, the percentage of sulphuric acid formed is very much smaller, the percentage of sulphurous acid formed remaining about the same. When the sulphur is heated rapidly with access of less air the proportion of sulphurous acid is increased while the percentage of sulphuric acid formed is very much lessened. It is of course difficult to carry on an experiment for collecting the gases formed which would at the same time simulate the conditions in a greenhouse where there is free access of air and the temperature is comparatively low. But the results of my experiments as given in the table would lead to the inference that a large percentage of sulphuric acid would be formed and very little or no sulphurous acid, as shown in experiments 8 and 9.

While it is known that sulphurous acid has marked germicidal properties and while a small percentage of sulphurous acid is produced when sulphur is heated under certain conditions, still owing to the large percentage of sulphuric acid produced when sulphur is heated under conditions which more nearly simulate those where sulphur is heated on steam-pipes in the green-house, the question arose as to whether sulphuric acid has not marked fungicidal action and as to whether it is not one of the active agents when sulphur is used as a fungicide. In order to determine whether sulphuric acid has fungicidal properties a number of experiments were carried out.

Spraying Experiments.

During the summer of 1904 some preliminary experiments were conducted to determine what strength of solution of sulphuric acid could be used for spraying plants without injurious effects. In this series of experiments the solutions were made with distilled water, and used in strengths varying from 1 part in 200 to 1 part in 10,000. The plants experimented with were the common field plants, such as wild cherry, elder, ailanthus, phytolacca, yellow dock, burdock, abutilon, blackberry, wild rose, milkweed, locust, etc. An ordinary

hand atomizer was used for the spraying operation, and the plants were usually sprayed late in the afternoon so as to avoid concentration of the solutions before they were absorbed, as also the burning effects of the sun. It was found that a solution of 1 to 200 produced a marked burning effect on the leaves, and in the case of some plants, as blackberry, polygonatum, oxalis, pear, rose, mulberry, raspberry, etc., contiguous parts of the plant exhibited signs of injury.

It was soon found that there is considerable difference in the resisting power of plants when sprayed in this manner. The most sensitive plants were peach, raspberry and wild carrot, while the most resistant were poison ivy and plantain (*Plantago major*). As a result of these experiments it was found that most plants could be sprayed with a solution of sulphuric acid varying in strength from 1 part in 500 to 1 part in 1000, without serious injury to the plants, and the conclusion was reached that it would be perfectly safe to use sulphuric acid solution as a spray in the strength of 1 part of acid to 1000 parts of water.

Later in the summer of 1904 it was my good fortune to find a rose garden in which some of the plants were badly affected with mildew and to obtain permission of the rose-grower to treat the affected plants. In the series of experiments a solution having a strength of 1 part of acid to 1000 parts of water was freely applied by means of a garden syringe, so that the plants were thoroughly wetted. Sprayings were made as follows: August 11 at 4 P. M., August 12 at 8 A. M., August 13 at 8 A. M., August 14 at 8 A. M., August 15 at 8 A. M., August 16 at 8 A. M.

Within about a week afterward the mildew had about all disappeared and the young leaves which were putting forth showed no signs of injury. During 1905 the mildew did not re-appear, and it was noticed that the plants which had been sprayed with the sulphuric acid solution the year before had never done better.

It may be of interest to state that parallel experiments using copper sulphate, 1 to 1000, were conducted with the result that while the mildew was eradicated the tips of the young leaves were injured.

During the winter of 1904–05 I had an opportunity of trying the solution on roses growing in the green-house. The plants were

sprayed with solution of sulphuric acid 1 to 1000, on December 7, 8, 9, 10, 12, 14, 16 and 19, with the result that the mildew was completely eradicated, the new growth was vigorous and the mildew did not reappear.

A few months later another lot of roses growing in the greenhouse were treated. These included pink rambler, crimson rambler, Victoria rambler, bridesmaid, golden gate, liberty, American beauty and Carnot. The plants were sprayed with sulphuric acid solution, 1 to 1000, on January 9, 10, 11, 12, 15 and 17, with results similar to those obtained in the previous experiments. I have also used the spray since with similar results.

In conclusion I may add that it appears that dilute solutions of sulphuric acid have a beneficial effect on the plants treated apart from their fungicidal action. In fact they seem to act as a tonic to the host plant while they kill the mildew.

Should subsequent experiments confirm the results here recorded, it will be found that sulphuric acid has certain advantages over sulphur, in that it does not discolor the foliage as sulphur does, its employment is more easily controlled, and it does not have the disagreeable odor of certain other compounds associated with sulphur.

Inasmuch as the antiseptic properties of sulphurous acid are well established, it may be possible that a combination of sulphuric and sulphurous acids in solution would be more effective than sulphuric acid alone. Taking the proportions in which these acids are formed when sulphur is heated, it may be that a solution containing 1 part of sulphuric acid and 0.1 to 0.01 part of sulphurous acid to 1000 parts of water, would be more effective than sulphuric acid alone in the proportion given.

Stated Meeting May 4, 1906.

President SMITH in the Chair.

Dr. David L. Edsall, a newly elected member, was presented to the Chair and took his seat in the Society.

Letters accepting membership were read from:

Hon. Joseph Hodges Choate, New York.

John W. Harshberger, Ph.D., Philadelphia.

William Francis Hillebrand, Ph.D., Washington.

Charles Rockwell Lanman, LL.D., Cambridge, Mass.

Ernest Fox Nichols, D.Sc., New York City.

Hon. Elihu Root, LL.D., Washington.

Thomas Day Seymour, LL.D., New Haven, Conn.

Edward Bradford Titchener, M.A., Ithaca, New York.

Otto Hilgard Tittmann, Washington.

Dr. Hendrik Antoon Lorentz, Leyden.

The decease was announced of Mr. Edward Floyd deLancey, at Spining, N. Y., on April 7, 1905, æt. 83.

The following letter was read from PROF. GEORGE DAVIDSON, of San Francisco, in relation to

THE SAN FRANCISCO EARTHQUAKE OF APRIL 18, 1906.

In the matter of the earthquake of April 18, while you were celebrating the two hundredth anniversary of the birth of Benjamin Franklin, I beg to give the following memorandum as a matter of record:

The shock occurred at $5^h\ 12^m\ 00^s$ A. M., Pacific standard time. The first movement was short and sharp, north and south, and of slight amplitude at my home 300 feet above the Bay; it changed to movements east and west and every direction, with very quick shocks (say four per second), of small amplitude and complex, for 60 seconds; when a slight but gradual weakening was apparent for 30 seconds more. After this the vibrations weakened gradually to quietness in another 60 seconds. The whole movements occupied $2^m\ 30^s$. I began my count with the first shock, and soon afterwards timed the event with my chronometer watch.

The sensation suggested the sharp vicious action of a terrier seizing and shaking a rat.

There was a slight shock at $5^h\ 17^m$ (approx.) and another at $5^h\ 29^m$ (approx.) and some later ones of no consequence.

In other parts of the city and the adjoining districts the amplitude of the shocks has been much greater, if we judge by results; and yet very unequal.

The general indications point to this statement: *That the greatest ampli-*

tude of the movement of the immediate earth surface is to be found in low alluvial situations or in made-land areas.

That was the deduction in 1868; with indications that at the extremity of a rocky ridge the full amplitude was seen.

The report of the 1868 Committee (of which I was a member), was never made public.

The Governor of California, George C. Pardee, has appointed the following persons members of a committee to investigate the phenomena of this earthquake: Professors A. C. Lamson, University of California (geology), G. K. Gilbert, U. S. Geol. Survey (geology), Harry Fielding Reid, Johns Hopkins University (geology), J. C. Branner, Stanford University (geology), A. O. Leuschner, University of California (geodesy, astronomy), George Davidson, University of California (geodesy, geography), Charles Burchatter, Chabot Observatory. This board has already met to confer on the plan of investigation.

I may mention here, incidentally, that the number of deaths officially reported as occurring from the earthquake and fire, is 277.

There is no epidemic; there is capital order, with the military in charge; and those who remain are stout-hearted; 125,000 encamped in parks, etc., are being fed.

This is the sixty-third earthquake I have experienced.

SAN FRANCISCO, April 25, 1906.

This communication was discussed by Professor Haupt, Dr. Goldsmith, Mr. Wharton and Mr. Richard Wood.

The following papers were read:

"Prof. de Vries' Theory of Evolution in Relation to Results," by Dr. Philip Calvert, which was discussed by Dr. Conklin and Dr. Calvert.

"The New Agriculture," by Mr. Burnet Landreth.

"The Present Condition of the Bar at Aransas Pass, due to the Operation of the Reaction Breakwater," by Prof. Louis M. Haupt.

"THE NEW AGRICULTURE."

By BURNET LANDRETH.

(*Read May 4, 1906.*)

The use of Peruvian guano and other concentrated fertilizers may be said to have been the initial steps leading to a new birth of agriculture.

Peruvian guano was introduced about one hundred years ago, but did not get into general use until about 1840, at about which time was introduced into frequent use ground bones and super-phosphate of lime, two manurial agents which have accomplished more than any other soil fertilizers, and at the same time into partial use nitrate of soda and nitrate of potash; all these objects of fertilizing force being greatly concentrated in comparison with the bulky fertilizers of earlier years, manures which still are highly valued, but such as cannot be profitably transported long distances. The use of these concentrated fertilizers in Europe increased the area of the sugar beet fields to an enormous extent as well as the area of everything else in the agricultural line.

In America, by these very rapid and cheap methods of adding plant foods to the soil, so-called fertilizer factories sprung up in all directions over the land and the crops of grain, cotton, tobacco, and other productions of the garden and field were doubled in product per acre as well as quadrupled in area. This subject of fertilizers, however, may be looked upon as a back number, or as hardly now to be classed in the process of "the new agriculture," but increased crops led up to other developments. Neither can steam plowing be looked upon as a new process of "the new agriculture," for it was practically pursued fifty years ago largely in England and Germany and to some extent in Egypt, but it broadened immensely the scope of agricultural practice. In America, however, steam plowing in early days never took a very prominent place, because until of late years there were few great American farming estates

operated by wealthy and very progressive men, it not being profitable to steam plow at one operation less than a field of forty or fifty acres, but of late years consequent upon the rapid extension of agricultural operations, especially in wheat growing, fields of grain have become so immense as to satisfy the most progressive. In England the system of plowing is distinct from what Americans have always clung to, even although up to quite lately with disappointing results. In Europe the system is altogether what is called the cable or rope system, where upon a field preferably not less than four hundred yards long a traction engine with a large horizontal revolving drum beneath the boiler has placed opposite to it at the other end of the field a corresponding heavily anchored revolving horizontal drum. Around these two drums passes a wire cable of about three fourths of an inch thick, which drags backward and forward a gang of plows of six, or even ten or twelve mould boards, an automatic device, after each set of furrows are turned, moving forward the anchor at the far end; the steam engine on the headland also moving forward to the same distance as the anchor, so that they are always on parallel lines. One engine and opposite anchor windlass forms what is termed the "single system," while in the "double system" there are two opposite engines.

These plowing outfits cost from ten to twenty thousand dollars, an amount which only can be assumed by a large operator. Not more than four or five of these outfits have been imported, one in the neighborhood of Philadelphia being imported by Colonel Joseph Patterson, who thirty years ago endeavored to establish a beet sugar farm and factory at Egg Harbor, N. J., which operation on account of unfavorable climatic influence was a failure, the plowing apparatus being subsequently sold to Wade Hampton, thence to Cuba.

Americans have wanted something cheaper, something easier to manage, and have spent large amounts of money in endeavoring to plow by *direct* action, using self-moving engines on three or four wheels pulling after them gangs of plows, just as a locomotive pulls a train of cars. This process up to a few years past met with so many difficulties, that really there were few thoroughly satisfactory applications of the American idea, for at seasons when the land is

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muddy and slippery the tractive force of the engine is reduced greatly, sometimes entirely nullified, at other seasons when the soil is dry and dusty, the engine kicks up such a dust as to ruin its own working parts.

Some years ago I had considerable experience in endeavoring to promote the direct traction system and devoted much of the three successive summers in 1872, 1873 and 1874 in an endeavor to do practical work.

I worked first with a three-wheeled Scott-Thompson engine with solid rubber tires, six inches thick for the purpose of increasing the tractive force, which tires flattened out under the weight of the engine as does a cat's foot. The two driving wheels were of a sixteen-inch face having clamped upon them segmental lags or blocks of rubber; as the whoeels revolved these rubbers flattened under the five-ton weight of the engine and gave a tractive grasp of the soil of quite four hundred superficial inches under each wheel. I also worked with a four-wheeled rigid-tire engine, the periphery of the wheel fitted with angle irons to increase the tractive power.

Succeeding these trials, or in the year 1887, General Roy Stone in my farm wheel-wright shop at Bloomdale invented and erected a steam digger of ten spades, a system established on a horizontal shaft, the spades jabbing into the earth directly in the rear of the engine. This required such an immense amount of power that we turned to something better; this being a rapidly revolving shaft actuating fifty or more cutting or chopping knives slicing off slivers of earth by a downward centrifugal movement just as a wood planing machine by a horizontal centrifugal movement chips off slivers of wood. These fifty knives, each on a distinct arm eighteen inches long, were fastened to a horizontal shaft revolving rapidly, throwing forward the various arms in centrifugal movement, the knives at the ends of the arms going down into the earth in oblique and curved direction, throwing the earth six feet behind the machine as a hay tedder throws back the grass, the knives always keeping open a broad trench beneath and behind them.

The land, when thus worked, was left as a perfect garden bed, needing no further working because it formed a bed of chippings ten

inches deep and perfectly level, every particle of soil cast back quite three or four feet from its original location; but the machinery was not perfect; no one at the time was ready to take it up; consequently, the engine was sold for another purpose and the chopping apparatus went to the scrap pile.

A photograph of the Bloomsdale steam chopper gives a partial idea of the best steam chopper ever used, and I take great credit that it was developed on my own farm. It was an ideal machine, but perhaps more theoretical than practical. This was in the years 1887 and 1888.

Since then there has been a great advance in methods of steam plowing by direct traction, and that by the western people, always leaders, if not in the invention, certainly in the adoption of all new machines and appliances, and they have not been laggards in perfecting the American idea as connected with steam plowing, for it has rested with a California establishment to most successfully promote the system of direct traction plowing.

This establishment makes a sixty-horse power traction engine which draws a gang of twenty-one earth-cutting disks, each cutting a foot wide and nearly a foot deep and doing in this style fifty acres a day, sometimes eighty acres.

In England no double engine cable system has ever done anything like this, and in addition the traction engine has sufficient power to drag at the same time a gang of grain sowers which seeds the same width of twenty-one feet. The engine makes its own electric light, so that operations can be conducted throughout the full twenty-four hours. One of these machines is in use in South Africa.

And there is another case of most remarkable development in agricultural mechanism, my reference being to harvesting machines, and this again by western people, who, by reason of their enormous breadths of ground and scarcity of labor, have always been very active in promoting the use of labor-saving machinery.

Many farmers in Minnesota and the Dakotas have for years been winning the admiration of the old east by using thirty and forty improved reapers at one time in the same field, all attended by traveling machine shops to effect repairs, but it has been left to a

Californian to devise a perfectly practical machine for cutting, threshing, cleaning and sacking all at one operation, the grain being made ready for market five minutes after the machine touches the field.

This machines requires the united power of thirty to forty horses, still better that of a traction engine, which by its electric light turns night into day. The machine will take care of five acres an hour or over one hundred acres in the twenty-four hours. These machines are used in California, Washington and Oregon, districts where the grain thoroughly matures on the stalk and where no rain occurs during the harvest season. The outfit costs $8,500, and one farmer in Spain has had nerve enough to purchase the full system.

And what other remarkable advances in farm implements, few of them as impressive as the steam plow or the combined harvester, but in their places equally as important. It would be too comprehensive a subject to endeavor to refer to them all, but as examples will name the corn stalk cutter and binder which handles a crop of corn much as a grain binder handles wheat or rye, or the corn husker which handles a standing crop of corn, assorting all stalks and husking the ears.

These mechaines are of special importance, for be it remembered, there is annually grown in the United States two acres of corn to every man, woman or child.

A later development of "the new agriculture," but not a novelty in practice, is the process of spraying or washing the stems and foliage of plants with liquids containing poisons for the destruction of insect life. This was introduced about 1870, and was at once found very practical in checking the ravages of many kinds of insects; but countrymen noted that there were certain other insects which were not killed; they observed some which seem to delight in a poison bath, some which put on the top surface of an open barrel of paris green seem to be more lively in the morning than they were the night before. This, brought to the attention of entomologists, elicited the information that there were two great classifications among insects which must be recognized by the farmer. Firstly, those which ate the tissues of plants, and, consequently, took into

their stomachs any poisons which accidentally or artificially rested upon the outer surfaces of the plants; while secondly, there were other insects which entomologists had recognized for one hundred and fifty years, but which the farmer had never critically observed, which did not masticate their food, those after the order of the mosquito, sap suckers, having a little pumping apparatus which they inserted beneath the cuticle of stem or leaf and drew out the sap, ignoring entirely any application of poison.

The entomologists advised the adoption of a different policy, saying that these sapsuckers must be strangled, and the way to do that would be to spray them with some liquid of an oily, caustic or soapy basis, because insects do not have lungs, in the ordinary understanding of the term, but breath through orifies along the sides of their bodies or abdomens sometimes covered by scales as in the case of the fish-like lamina of the honey bee. The entomologists told the farmer that an oily spray put upon these or, indeed, any other insects, would gum up the orifices and the insects, consequently, die. Thus, between the surface application of poisons for the tissue-eating insects and the application of oily fluids for the sap suckers, most insects can be kept in check.

The insect now attracting most attention on the part of the fruit growers is the San Jose scale, a native of China, believed to have been brought to California on imported trees. These insects have by gradations of progress eastward covered the entire country, and if left alone would destroy every fruit tree in the land. But very practical steps are now being taken everywhere to eradicate this insect by the application of a liquid combination of lime and sulphur, which is caustic in its action, killing most of the half-grown hibernating scale insects in winter, and preventing the settling of any young that may come from the few parent insects escaping the wash.

As much as forty years ago scientific men indicated to seedsmen the use of several insecticides, principally carbon-bisulphid for the treatment of weevil-infected seeds, or for the treatment, in fact, of any seeds bearing on their surfaces or within them insects or mites. Seedsmen and grain merchants are especially annoyed in the conduct of their business by the depredations of the weevil family which

burrow or ensconce themselves notably in the seeds of peas or beans, corn or wheat. These grubs may be killed in their holes by the fumes of carbon bisulphid, to effect which the seedsman erects a room say thirty or forty feet square, lined top, bottom and sides with tin and with a door which can be hermetically sealted. Such a room can be loaded up with a carload, say six hundred bushels of peas, beans or anything else.

A carload of peas or beans thus treated and subjected to the fumes from a gallon of the liquid for thirty hours may then be taken out with all the larva killed, the fumes penetrating not only to the center of the pile, but to the center of every sack. The odor of the fumes soon dissipates, leaving no resultant injury to seed vitality nor to edible properties of either seeds or food stuffs: A more efficient agent is hydrocyanic acid gas, but it is dangerous in the hands of ignorant people.

About 1860 the use of carbon bisulphid was introduced among the vineyardists of France as an agent to arrest the ravages of the phylloxera, a plant louse feeding on the roots of the grape-vine. Possibly at this date 300,000 acres of vineyards are being annually treated, a half ounce of a liquid being applied by injection to every square foot of soil surface, the vapor filling all the soil interstices, the application being two treatments of ten days apart.

But an entirely new practice based upon scientific observations and a rather amusing contribution of science to agriculture is the introduction of cannibal bugs, sorts which do not injure vegetation, but luxuriate on the meat of other species of bugs. Some of these precious insects have been imported from China and are doing a fair amount of work on the California coast, while certain imported Mexican ants are very active in Texas cotton fields, feeding upon the cotton boll worm and its eggs. Thus science, while going hand in hand with agriculture, encourages cannibalism.

The farmer again had to turn to scientists to learn how to arrest the injuries from fungus diseases on his growing plants of grain, vegetables and fruits, and there has grown up a large industry in the manufacture of fungicides, principally copper compounds, which are sprayed upon the plants the same as are the insecticides. In

fact, as a matter of economy of application, both fungicides and insecticides often can profitably be mixed and the plants treated by one operation. All designs and sizes of apparatus for the application of insecticides and fungicides are now made ranging from the small air gun to cumbrous four-horse trucks carrying tanks and powerful pumps and requiring the attention of several men—some forms of dust spraying apparatus being made for hillside use, where it would not be practical to haul great weights of water.

The electric light as a forcing agent is used to advantage in stimulating the growth of vegetables and flowers. Some market gardeners in the vicinity of Boston, Mass., finding that its use pays them a profit by influencing a never-ceasing growth of lettuce, radish, cucumber and some other table vegetables, the plants not being allowed any rest day or night, but kept under a continuous activity which in the end amounts to about a ten per cent. shortening or hastening of the period of maturity, that is to say, plants which under ordinary circumstances would take thirty days to arrive at marketable condition, will under the electric light process be fit for market in twenty-seven days, which earlier maturity results in a profit, and allows space and time for an additional crop in the year's series.

These Boston men also apply continuous currents of electricity passing from end to end of plant beds in their glass houses, which is found to have a stimulating effect on the productivity of the soil. They have successfully accomplished what many others have failed to do. Even as far back as 1866 electric stimulation of the soil was tried most extensively and expensively. In that year I visited Tipptree Hall, the estate of Alderman Mechi of London, where were conducted most extensive electric experiments on field crops covering broad areas, but the so-called electric experts of that day failed entirely, although Mr. Mechi was lavish in his expenditures.

Nitroculture while several years pursued may be classed as one of the novelties in agriculture practice.

Ages ago the ancients were aware that certain plants increased the fertility of the soil, as for example clover, beans, vetches, and other legumes, but the most intelligent did not know why, simply

knew of the favorable results. Later on, intelligent observers concluded that it was because of the deep rooting habit of plants of the legume family; that it was entirely the result of the collection out of the three, four or five feet depth of soil to which the roots reached, of all the potash there which was seized upon and drawn up by the roots and concentrated near the crown within an inch of the surface, and, no doubt, to an extent the theory was correct, but scientists now tell us a new story and a most interesting one. No intelligent farmer has failed to observe upon the roots of his clover or bean plants that occasionally there is presented to his view a something which looks like a diseased condition of the roots, a growth of warts or nodules, sometimes four or five to a plant and sometimes a hundred. Occasionally this will occur at one end of the field and not at all on another field.

The bacteriologists now tell us that these nodules are caused by the attachment to the roots of plants of the leguminosæ family of certain bacteria, that these bacteria within the resulting nodules or swellings absorb out of the air of the soil a portion of its introgen and store it for the support of the growing plant itself, as also for the succeeding year's crop and this stored nitrogen is now said to be the principal secret of the imparted fertility to soil by the cultivation of a crop of clover.

Some parties are now commercially developing these microorganisms, offering for sale in small sealed tubes portions of the living cultures sufficient to develop an indefinite quantity, just as a yeast cake which one may carry in his pocket may leaven sufficient dough to inoculate a large amount. The commercial preparations being mixed as directed in water, the solution may be used to inoculate as much soil as the solution will moisten, and this soil afterwards mixed with four or five times its bulk, and the whole thinly spread broadcast over that portion of the field to be inoculated; or the system now most generally advised is to inoculate the seed before planting it by subjecting it to a spraying or a bath. The seed may be partially dried before sowing, or sown in its wet condition.

The practice so far, to a large extent, is experimental, there being various degrees of success and failure, but the day is close

at hand when better results will be obtained. Little benefit results to any particular crop if the bacteria of that crop is already present in the soil, and little benefit if the soil is already well charged with nitrogen, or very deficient in potash or phosphate. Inoculation is only one factor in securing a successful growth of legumes and unless the soil conditions are favorable to the multiplication and activity of the bacteria, inoculation will be ineffectual.

Now as to the use of chloroform in plant culture. This is a process, only a few years introduced, of forcing the blooming of plants away ahead of their natural period, as for example making lilacs, azaleas, lilies of the valley, hyacinths and violets burst into bloom at Christmas time instead of their natural period about two or three months later. This system is well established in Germany, Belgium and France, and will, no doubt, be practiced in all parts of the world.

It is not a new thought to say that all plants must undergo a season of repose, a repose induced by some internal and external force, the plants seemingly being dead, there being no perceptible movement whatever excepting a slow increase in the size of the buds. All country people have observed that after a fruit tree loses its leaves unusually early in the autumn that it sometimes bursts into bloom before winter; it has had its season of repose, a short season it is true, but it undertakes then to proceed with further development. Now to force or intensify an early repose or sleep, chloroform is used. The susceptibility of plants chloroformed is an interesting discovery, all plants being similarly effected much as in the case of animals; it is even claimed chloroform puts metals to sleep. A test can easily be made with a sensitive plant or mimosa, placed under a bell glass with a small sponge dipped in chloroform; after a few minutes the plant will be observed to have lost all its sensibility, but when exposed to the open air, the sensibility returns. The exposure of plants to chloroform produces on short notice a most intense and deep rest, similar to that after the autumn shedding of their foliage, but a rest far deeper than under the ordinary circumstances. It has been observed that plants which had been exposed to cold and dry winds are likely to bloom early, alpine plants for example, and it is observed that the action of chloroform

produces a drying up of the tissues rendering the buds quickly susceptible to subsequent heat and moisture. This discovery opens up a broad field for the producing of blooms ahead of time as also a guarantee for the gathering of early crop of fruit. Of course, these processes can best be done in properly prepared chambers or glass houses, but, no doubt, means will be devised for covering outdoor plants with something similar to hugh bell glasses for treatment by chloroform.

One of the latest scientific discoveries in connection with agriculture is that of the influence of electric air currents. Travellers in far northern regions have noticed a wonderful rapidity of growth and brilliancy of coloring to the arctic flowers and a strength of perfume. They have noticed that succeeding a snow surface there will, forty or fifty days after its disappearance, be developed a growth of natural grass, as tall as would be produced in four times the period in temperate regions and this on poor soils. They have noticed that these growths are even still more astounding throughout spring months of unusual auroreal disturbances. Usual auroras occur about once every ten years, and in those the growths of all vegetation is far in excess of the ordinary years. This has been especially noticed in northern Sweden and Norway. Cutting down a tree there, it is observed that the annual rings about every ten years are much broader than those before and after. All this has led close observers to believe that these unusual growths are a consequence of electrical air currents, and with that theory in view most interesting experiments have been conducted proving that the theory is correct.

Professor Lemstrom, of the University of Helsingfors, was the first in these experiments, which experiments have been pursued at Avidaberg in Sweden and at Kryschanowitz in Germany and at Durham College in England, all proving conclusively that electric air currents greatly forward the growth of plants. In some cases not only ten and twenty per cent. increase in productiveness, but in other cases sixty and eighty per cent. increased development. Strawberry plants subjected to electric air currents are increased fifty to sixty per cent. in productiveness and sugary qualities and just so

with other garden crops. It has been discovered that the electric current is ineffectual on a wet and cloudy day, and positively injurious on a very dry day.

The processes of application may be generally described as follows: A plot of ground, say forty by forty feet or larger, for example, a portion of a strawberry patch, is covered by a wire netting two feet above the level of the crop insulated on wooden posts; an electric battery upon wheels is moved up close to the edge of the wire netting and the wire of the positive pole attached to the iron netting, the negative pole plunged into the earth; the electric machine is started and alternating currents are passed backward and forward, jumping from the wire netting to the leaves and conversely from the leaves to the netting. This application of electric currents repeated at intervals, the wire netting being moved from part to part of the crop, until in the end the whole field has finally been treated. Of course, this system cannot be used over very broad surfaces, but it has been put into practical use on areas as much as of three acres. Professor Lemstrom estimated the cost of an efficient electrical machine at $500 and $150 as the annual cost of the material and extras.

A still newer, and probably the newest scientific application in the line of "the new agriculture," is the electrocution of insects, a system just patented by an electrical engineer at Odessa, and in this connection I will relate an interesting incident.

Twenty years ago I was a special correspondent of a European Ministry of Agriculture upon subjects concerning the development of agriculture in America, and on one occasion, when I was asked to report upon the phylloxera in the United States, I supplemented the report by a suggestion that it might be possible to kill the phylloxera by a current of electricity passing through the stems and roots of the grape vine. The Ministry of Agriculture in office at that date thanked me for the suggestion, but said it was impractical; yet, strange to say, this Russian electrical engineer has taken out patents for doing this very special thing. If this be so, then by electricity, capable, on the one hand, as a mighty force to rend a mountain, or of a movement soft as zephyr, it may be possible yet to destroy every bug on tree or plant, whether in the open field or in glass houses.

What I have said are generalizations very incomplete, yet they, nevertheless, indicate that the practice of agriculture and horticulture has taken a highly mechanical and scientific tone, a pursuit worthy of the most intelligent mind.

Stated Meeting May 18, 1906.

President SMITH in the Chair.

Dr. J. W. Harshberger and Mr. Russell Duane, newly elected members, were presented to the Chair and took their seats in the Society.

A letter was read from Prof. Theodor Nöldeke accepting membership.

The decease was announced of the following named members:

Richard Garnett, C.B., LL.D., at London, on April 1, 1906, æt. 68.

Prof. E. Renevier, at Lausanne, Switzerland, on May 5, 1906.

Hon. Carl Schurz, at New York, on May 11, 1906, æt. 77.

The following letter was read by PROF. GEORGE DAVIDSON, of San Francisco:

POINTS OF INTEREST INVOLVED IN THE SAN FRANCISCO EARTHQUAKE.

At the request of your Secretary I draw up this short account of some of the points of interest involved in the earthquake and conflagration of San Francisco.

I hardly know what I have already written, or where to begin to tell a coherent story, although a thousand facts have been seared upon my brain. I see the flames rising yet, 500 feet above the earth.

It really seems very long since the earthquake aroused us: it would have been forgotten by this time. It is the conflagration, its awful grandeur, and its ruin of tens of thousands of people, that staggers us with its horror, and its consequences. But San Francisco will arise from this supreme blow. We are to control the Pacific.

I cannot do better than to first give you a broad idea of the geography and orography of this part of the State; and of the ocean washing it. The general direction of the coast range of mountains is northwest and southeast from Cape Mendocino in latitude 40° 27′ to Point Conception in 34° 27′. North of latitude 35½° the range comes sharply to the ocean in two or three

high, sharp, rocky plications, folds or ridges, with regular or irregular valleys between them; and *broken down* (1) at the Bay of Monterey in latitude 36° 36′; (2) at the Golden Gate in 37° 49′; and (3) at Bodega Bay in 38° 18′.

Inside of this coast range lies the Great Valley of California, having an average width of 65 miles, 450 miles in length, and bounded on the east by the Sierra Nevada.

The *ocean face* of the coast mountains is high, rocky, bold, and grand in certain parts, for example: one of the Twin Peaks of the Santa Lucia range in latitude 36° 03′, rises to 5100 feet only three miles from the sea; Mt. Santa Lucia about ten miles east, with deep intervening valley, rises to 6100 feet; Loma Prieta (formerly Mount Bache on Coast Survey charts) is in the San Francisco Peninsula range in latitude 37° 07′ and reaches 3793 feet above the sea; Tamalpais Mount in the range on the peninsula north of the Golden Gate in latitude 37° 55′, and only four miles from the sea, is 2604 feet high. To the westward of this mountain is the outstretching Point Reyer Head 597 feet high, stretching eight miles beyond the general direction of the coast and connected therewith with low-lying alluvial lands.

North of Bodega Head (38° 18′) the general height of the range is 2200 feet, until within twenty miles of Cape Mendocino, it reaches 4265 feet at Krag Peak latitude 40° 09′.

Beyond Cape Mendocino (40° 27′) the range is continued under the sea, as shown by a few deep sea soundings. It is here that ships have experienced the effects of submarine earthquakes.

The greatest *transverse line of rupture* in these ranges is the Golden Gate, one mile wide and sixty-three fathoms deep. A less but longer line of *low rupture* is from Bodega Bay to Petaluma Creek that empties into the northwest part of San Pablo Bay.

Ocean Depths.—Off this bold and rocky coast the profound depths of the Pacific lie close aboard.

In 1874 Captain George Belknap, U. S. Navy, ran several lines of deep sea soundings broad off certain points of the coast to the depths of 1000 and 2000 fathoms, so that we know the depth of 1000 fathoms is found at 40 to 60 geog. miles from shore and the deep plateau of 2000 fathoms, 2¼ miles, at 46 to 72 geog. miles. The shortest distance is off the Santa Lucia range: the middle off the Gulf of the Farallones, and the longest off Cape Mendocino. At the latter the 1000 geographical miles sounding was gotten at 25 miles; and the 2263 sounding at 85 miles.

And it should be noted that off the high bold coast just south of Cape Mendocino there are *four submarine, or submerged valleys* that head directly at the shore under the highest peaks, and at right angles to the coast. (See my paper on Submerged Valleys.) Those are the more general features of the coast line. The coast survey charts exhibit the line of coast, the heights, and the depths.

The Bay of San Francisco.—This bay lies in one of the valleys of the coast range, and therefore is mainly parallel therewith; and the great transverse break through this range helps to form the bay.

The line of Suisun Bay, Karquines Strait, San Pablo Bay, and the Golden Gate is the course of the drainage of the Great Valley of California, and the adjacent valleys. It is the natural highway of commercial activity between the interior regions and the Pacific.

The area of the Bay proper is 301 square miles, of which 83 square miles have four fathoms and more of water; San Pablo at the north has an area of 123 square miles, of which 17 square miles carry over four fathoms of water.

Both these bays are largely bordered by broad, low, salt marshes, cut by many sloughs. Inside of this marshland (which marks the shoreline of high water) the low, alluvial soil may be from one to six miles wide, to where it joins the foot of the rocky ridges. In places the rocky points reach into deep water.

Some of these features are well shown on the coast survey chart "*Entrance to San Francisco Bay*"; and Professor J. D. Whitney's map of San Francisco Bay (1873) will give a general idea of the confining ridges; and the cross breakings.

There are two features that are particularly noticeable: (1) the subordinate valley running from Bodega through Tomales Bay and Valley to Bolinas Bay. The heights on the southwest side of this narrow valley reach 1356 feet in height; and on the northeast reach 1500 to 2604 feet.

(2) On the prolongation of this valley, across the Gulf of the Farallones, there commences a narrow valley about six miles below Point Lobos, and stretches southeastward, as a line of depression on the east side of the crest line of the Peninsula of San Francisco range, for thirty-six miles in a direct line. In this long stretch lie several small lagoons, and the Cañada de Raymundo where the Spring Valley Water Company has its main reservoirs. Of course the bottom of this valley is alluvial soil. The water company has two reservoir dams across it, and a third at the exit of drainage through San Mateo Creek or Cañon to the bay on the east. Neither of these dams is reported injured. Professor Branner, of Stanford University, has found some earthquake crevices in the line of depression. No rock rupture is reported there.

With regard to alluvial soil, it is well to bear in mind the relation of this whether in the valleys or around the bay and ocean shores, and the relation thereof to the rocky base upon which it rests or abuts.

I was one of the committee of investigation of the 1868 earthquake, and it demonstrated that the *course of greatest dislocation at the surface of the ground was on the line of contact between the "made" land or the alluvial soil with the rocky stratum.*

This is repeated in this earthquake, and therefore we make a few more remarks upon the bay shore line of San Francisco.

In the vicinity of the city proper, and within the area of the "City and County of San Francisco" (42.8 square miles), there are several indentations of the shore between rocky points. (1) Buena Cove, where the present "city front" or shipping point lies on "made" ground; (2) Mission Bay, Creek and Lagoon, being filled in; (3) Islais Creek, farther south.

In Buena Cove the mud has been found ninety feet deep; in Mission Creek ninety-six feet. The "filling in" has been sand, the waste of the city, and occasionally rock. To erect buildings thereon piling must be driven to "hard pan." In Mission Creek this has rarely been done; and therefore we must expect to find, and do find the greatest dislocation in such areas, and at the contact with fast land.

The towns of Redwood, Palo Alto, San José, etc., are built on the alluvial soil immediately inside the marshland, and are probably twenty feet above the bay.

These places and others about the bay show large destruction of property.

Up the Valleys of Petaluma and Napa similar results are presented.

On the rocky parts of San Francisco the horizontal resultant movement was three to four inches, on the "filled" in land near the new Post Office, the building, piled upon a clay foundation, suffered but little, while the street in front, made upon twenty-three feet of sand over a marsh, sank two feet and slid out from the building about one and one-half feet.

Area of Disturbance.—The Committee appointed by the Governor to gather data is doing what it can to define the locus of principal action, and the extent and direction of the lines of disturbance.

In my early inquiries I learn of injury to the Light House at Point Arena, latitude 39° 00′, 110 miles from San Francisco, and at "landing places" on the coast this way; damage fifty to sixty miles up the Petaluma and Napa Valleys; movement in the Yosemite Valley, 175 miles from San Francisco; and damage at Carmel Bay south of Monterey, ninety miles from San Francisco.

Up the Napa Valley material was thrown to the west-southwest; at Yosemite motion east and west; at Carmel Bay material to east; at Carson, Nevada, first movements nearly east and west, distance 180 miles to northeast, ended in several tangles.

At San Francisco movement from north to south, then east and west; and a final series of reverberations too puzzling to decipher. I judged these shocks to be about three or four per second.

There are reports of crevices in the west slope of the hills north of Berkeley; in the Cañada de Raymundo, with depression on one side; and on the outer coast at Half Moon Bay, twenty miles south of San Francisco; and at Bolinas Bay ten miles north of the Golden Gate. But all are in alluvial soil.

Vessels at Sea.—The action upon vessels in the Gulf of the Farallones, off the entrance to the Golden Gate, has been given me by Pilot Hayes. The Pilot boat Gracey S. was lying in eighteen fathoms off the light ship, ten miles from the Head of the entrance to the Golden Gate, and shivered as if the chain were running through the hawse. When the pilot boarded the German ship the captain told him he thought his vessel was on the rocks; and a second pilot boat thought that she was upon the rocks.

This striking and shivering of the ship has been felt many times off the northwest of Cape Mendocino, or the prolongation of that range.

That is a very unequal description of the region affected by the earthquake and its results. When we get a coherent report, a copy will be sent to the Society.

I wish in conclusion to say a few words of the conflagration that covered an area ten times greater than that of 1666 in London, with a vastly larger proportion of loss.

1. The electric power lines running to hundreds of industrial plants in the city are believed to have been broken, and that a fire was started at each break as if by magic. All the city fire engines were called out but *found no water.*

2. Where the 30-inch main crossed the "made" or "filled" in land at the head of the old Mission Lagoon, there was a sinking of the material, and the joints were drawn asunder at a drop of several feet. Therefore the main supply was unavailable; and the reservoirs ran to waste. Then the water was pumped from Lake Merced; and finally some repairs were made and the water was gotten to help the dynamiters when the fire had really crossed the wide Van Ness Avenue. The Claus Spreckels brownstone mansion on the west side stopped its progress to the northwest.

At our elevation of 340 feet we got *no water for* two weeks; no lights except candles yet; no fires in houses; and the people are cooking in the streets; rich and poor.

That is the gist of the matter so far as I can gather without having official authority. We expect much from the Light House Board; from Coast Survey tidal observations, and other sources.

Area of the burned district.

4 square miles; say 2560 acres
London 436.

490 blocks

and probably 25,000 houses

SAN FRANCISCO, May 10, 1906.

This letter was discussed by Prof. W. B. Scott.

The following papers were read:

"The Nutritive Requirements of the Human Body," by Prof. Francis G. Benedict, which was discussed by Professor Houston, Professor Haupt, Mr. Wilcox, Mr. Goodwin and Professor Scott.

"A History of the Major Classification of the Mammalia," by Prof. Henry F. Osborn.